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A COMPARATIVE MICROSCOPIC STUDY OF THE MELANIN CONTENT OF PIGMENTED SKINS WITH SPECIAL REFERENCE TO THE QUESTION OF COLOR INHERIT- ANCE AMONG MULATTOS

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INTRODUCTORY, WITH A STATEMENT OF PROBLEMS INVOLVED

THE comparative histologic study of pigmented skins was undertaken with the hope of discovering evidence that might throw more light on the problem of color inheritance among the descendants of crosses between whites and negroes. The primary point at issue, until very recently, was whether human skin color in inheritance conformed more closely to the alternative (Mendelian), or the blended scheme; or perhaps to the ancestral (Galtonian) scheme. The studies of G. C. and C. B. Davenport show conclusively that there is a measure of segregation among the individuals of the third generation, hence a Mendelian-like inheritance.

This study, suggested to me by the above-mentioned investigators, is, consequently, more especially an attempt to test, as critically as microscopic data will allow, the theory of discrete unit characters in color inheritance (discontinuity theory; segregation theory), as opposed to the theory of continuity of the pigment character with interruption of the pigmentation process at

various stages. The study involves two incidental problems: (1) Source of the epidermal pigment, (2) cause of degree of coloration of skin. These matters must receive consideration first. Then follows a discussion of the bearings of the determined facts on the question of color inheritance in crosses between whites and blacks, or mulattos.

Material.—The material studied comprises 18 pieces of skin taken from near the mid-line of the abdomen, including 5 pieces from full-blooded negroes of varying grades of color, 6 pieces from mulattos of various shades, 4 pieces from brunets, 2 from blonds, and 1 piece

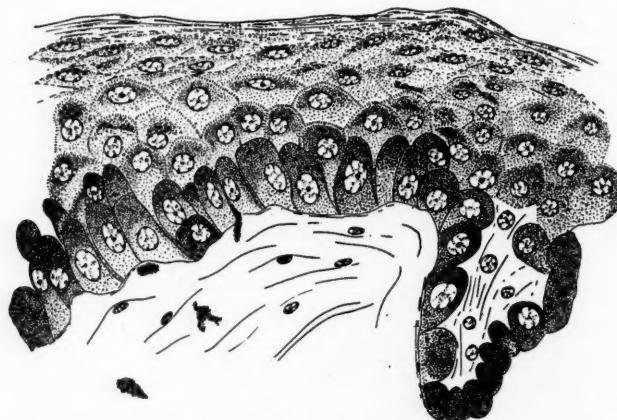


FIG. 1. Camera lucida drawing of unstained section of negro skin (specimen No. 14), showing the distribution of the pigment granules in the epidermis. Nuclei drawn according to their appearance in stained preparation. Dermal pigment cells very much more abundant than would appear from this particular region. The greater height of the basal cells of the rete mucosum Malpighi, as compared with Figs. 2 and 3, due in part to slight obliquity of section. The eleidin and keratohyalin granules of the stratum granulosum not shown. $\times 750$.

of pathologically pigmented skin; also a melanotic sarcoma, several pieces of pigmented skin of incomplete history, sections of infant's scalp and eyelid of newborn mulatto. Individuals were classified as blond or brunet according to color of hair. Mulattos were so adjudged in most cases from general appearance, *i. e.*, absence of distinct negro features, *e. g.*, thick lips, flat nose, etc., though in several cases the individual con-

tributing the skin admitted to me being "mixed." All but one piece of mulatto skin were tested by the Bradley color top before embedding. In only one case (No. 2) could the individual contributing negro skin be questioned by myself in regard to purity; the remaining four pieces are so classified on the assurance of pathologist and surgeon. Four pieces were tested with the color top. Sections were cut both in celloidin and paraffin, and studied unstained and stained (with 1/12 oil immersion lens) in carbol fuchsin or the hematoxylin and eosin combination. The following is a list of the specimens and the essential points of interest regarding each:

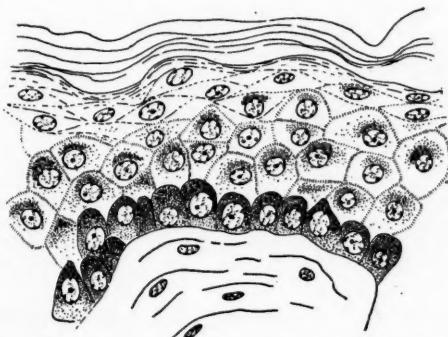


FIG. 2. Drawing of section of light brown mulatto skin (specimen No. 3) showing distribution of the pigment granules in the rete mucosum Malpighi. Darker and lighter mulatto skins differ from this only in the greater and smaller number, respectively, of melanin granules. $\times 750$.

1. Pure negro; from postmortem by Dr. H. Marshall (professor of pathology, University of Virginia); Zenker's fixation; color (matched by mixing yellow, white, red and black on the Bradley color top)—yellow, 4 per cent.; white, 8 per cent.; red, 22 per cent.; black, 66 per cent.
2. Pure negro woman; from abdominal operation by Dr. Harvey Stone (adjunct professor of surgery); fixed in 95 per cent. alcohol; a shade lighter than No. 1.
3. Light mulatto woman; abdominal operation by Dr. Stone; 95 per cent. alcohol fixation; color—yellow, 10 per cent.; white, 18 per cent.; red, 22 per cent.; black, 50 per cent. (Fig. 2).

4. Light mulatto; abdominal operation by Dr. Stone; 95 per cent. alcohol fixation; a shade darker than No. 3.
5. Blond; abdominal operation by Dr. Stone; 95 per cent. alcohol fixation.
6. Pathologically pigmented (grayish yellow) skin of white individual; postmortem (thyroid adenomata) by Dr. Marshall; 95 per cent. alcohol fixation.
7. Brunet; abdominal operation by Dr. W. H. Goodwin (adjunct professor of surgery); 95 per cent. alcohol fixation.

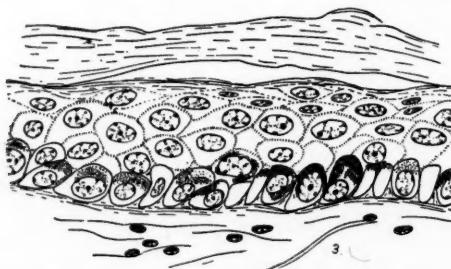


FIG. 3. Drawing of section of skin of blond ("cross between blond and brunet"—specimen No. 13). In lighter blonds (e. g., specimen No. 5) fewer basal cells contain still fewer granules; in darker brunets the basal cells contain more granules. $\times 750$.

8. Light mulatto male (age 80); postmortem (acute cystitis) by Dr. W. Thalhimer (instructor in pathology); Zenker's fixation; color—yellow, 10 per cent.; white, 17 per cent.; red, 30 per cent.; black, 43 per cent.
9. Pure negro; abdominal operation by Dr. Stephen H. Watts (professor of surgery)—95 per cent. alcohol fixation.
10. Brown mulatto boy; abdominal operation by Dr. S. H. Watts; 95 per cent. alcohol fixation.
11. Brunet; abdominal operation by Dr. S. H. Watts; 95 per cent. alcohol fixation.
12. Brunet female (gray); postmortem (goitre and old age) by Dr. W. Thalhimer; Zenker fixation.
13. Dark blond male—age 30 years—"cross between blonde and brunet"); from postmortem (pyæmia and perinephric abscess) by Dr. W. Thalhimer; 95 per cent. alcohol fixation (Fig. 3).
14. Pure negro; abdominal operation by Dr. S. H.

Watts; 95 per cent. alcohol fixation; color—yellow, 5 per cent.; white, 7 per cent.; red, 19 per cent.; black, 69 per cent. (Fig. 1).

15. Pure negro; abdominal operation by Dr. S. H. Watts; 95 per cent. alcohol fixation; slightly lighter than No. 14.

16. Brunet; abdominal operation by Dr. S. H. Watts; 95 per cent. alcohol fixation.

17. Mulatto; abdominal operation by Dr. S. H. Watts; 95 per cent. alcohol fixation; color—yellow, 14 per cent.; white, 14 per cent.; red, 35 per cent.; black, 37 per cent.

18. Mulatto; abdominal operation by Dr. S. H. Watts; 95 per cent. alcohol fixation; a shade lighter than No. 17.

Ranged according to degree of coloration, judged macroscopically, the 17 pieces of skin (omitting No. 6) take the following order:

Negro 9, 14 (B, 69), 1 (B, 66), 2, 15.

Mulatto 4, 3 (B, 50), 8 (B, 43), 17 (B, 37), 10, 18.

Brunet 7, 11, 12, 16, Blond 13, 5.

This list could have been enlarged indefinitely, but more material was not deemed requisite to the demands of this study. Number 6 took rank between numbers 18 and 7.

Sources of Epidermal Pigment.—There are obviously three distinct views which might be held—and as a matter of fact have been held—in regard to the origin of the pigment of the skin: (1) In the epithelial cells of epidermis; (2) in the connective tissue cells of the dermis, and secondarily transferred to the cells of the rete mucosum Malpighi; (3) in both the epithelial and connective tissue cells. The second view more especially further involves the question as to whether the pigment arises in the connective tissue cells as a result of cellular (secretory) activity or whether the cell extracts the pigment (fully formed or unsynthesized) from the blood; also the manner of the transference of the pigment to the epithelial cell.

There are two works which bear upon this point more directly: (1) That of Karg (1888) who ably supports the second view on the basis of findings from a microscopic

study of white skin transplanted to a negro and *vice versa*, *i. e.*, negro skin transplanted to a white individual; (2) that of Meirowsky (1908), who studied the origin of melanic pigment in the skin and eye and, more especially, on the basis of experiments with pigeons and findings in pigmented skin kept alive for several days in a paraffin bath at a temperature of 56 degrees, urges the first view. These two masterly papers advance diametrically opposing views. Karg unqualifiedly put aside the idea that pigment may be formed within the epidermal cell. He says, "Es gelang so, festzustellen, das es nur eines Modus der Entstehung des Pigments giebt. Aus der Lederhaut dringen pigmentirte Zellen in die Epidermis, verästeln sich hier weit und geben ihr Pigment an die Epithelzellen ab," p. 370. Meirowsky describes conditions more in conformity with our later ideas of cellular physiology. According to him, the melanic granules are passed out of the nucleus of the epidermal cell into its cytoplasm, *i. e.*, pigmentation is an intracellular process, both dermal and epidermal.

Historically Riehl (1884) appears to have been the first to describe the condition afterwards interpreted by Karg as supporting his view of epithelial pigmentation, viz., invasion of processes of pigmented connective tissue cells among the epidermal cells. Riehl studied more especially the pigment of hair. He did not generalize, however; and while he thought it improbable, he did not regard it impossible, that pigment may arise out of the protoplasm of epithelial cells. He seems rather to incline to the third view above stated. Moreover, on the basis of findings in a study of three cases of Addison's disease, where the adventitia of blood vessels was richly infiltrated with pigmented cells coincidentally with a hemorrhagic aggregation of red blood cells, he concludes that the pigment arises from the haemoglobin of the red cells. Aeby (1885) likewise describes the transportation of pigment to the epidermis by wandering cells. Ehrmann (1885-86), who studied the pigmented epithelium of amphibia, differs from Riehl and Aeby only in holding that the pigmented stellate cells of the cutis

are non-motile. He describes a network of pigmented cutis cells (chromatophores) connected with the epidermis by processes through which the pigment granules stream into the epithelial cells of the rete mucosum Malpighi. He also concludes that the pigment arises from the hæmoglobin of the red cells, since the pigment cells are most abundant in the vicinity of the blood vessels. The reception of the pigment granules by the epithelial cells he regards a phagocytic process, the epithelial cells being described as corroding the processes and assimilating the contents as part of their own organization. The important observations of Jager (1885) on pigment spots in dog and rabbit after inflammation, and those of R. Krause (1888) on apes, are in substantial accord with those of the afore-mentioned investigators, more particularly in regard to the secondary origin of pigment in the epidermis, and the primary source of the same in the blood. For further information regarding literature of pigment cells, more particularly in the lower vertebrates, the reader is referred to the splendid article by Karg.

It remains to outline more fully Karg's position as representative of the second view above stated. Pigmentation of epidermis and its appendages (hair, etc.) is a secondary process. The pigment is transferred to the epithelial cells through cells which have their origin in the cutis (*i. e.*, chromatophore, Ehrmann). They wander into, or, remaining on border line between epidermis and cutis, send processes into the intercellular spaces of the epidermis. They end in the epithelial cells (capable of a certain amount of amœboid motility) to which they surrender their pigment through process of absorption causing streaming from process to cell. These pigmented cells are wandering cells of the nature of connective tissue cells. They are thought to obtain their pigment from the blood. This, however, is not regarded as hæmoglobin since no red corpuscles are ever seen in these cells. There is here (*i. e.*, in transplanted human skin) no network of pigment cells as described by Ehrmann in amphibia. Nor can the pigment cells be

pigmented leucocytes since they have no similarity to the latter, nor are pigmented leucocytes found in negro blood. In the cutis he claims to be able to see all the transition stages between pigment-free connective tissue cells and such as are strongly pigmented. On the basis of his findings he thinks it appears only reasonable that in the pigmenting skin (transplanted white) the earlier unpigmented granules (Reinke's trophoplasts; Alt-mann's bioblasts) of the chromatophore take on a dark color (by as yet unknown ways) and that this is connected with the presence of blood; and that it can not, however, be regarded as the product of fragmentated red corpuscles. Melanin may be hemosiderin, but is the product of a living cell. His final position thus approaches somewhat to Meirowsky's, though they still differ as to the cell that elaborates the pigment for the epidermis. It is important in this connection to note that while Karg describes numerous branching pigmented connective tissue cells (wandering cells) among the epidermal cells of the transplanted white skin, he failed to find such in the normal skin of the negro. He thinks it probable that such may be found in the skin of the negro embryo.

Meirowsky in his monograph also gives a very complete review of the literature to date. He uses experimental methods mainly. His findings supporting the first view may be briefly summarized: Pigmentation is possible in the epidermis without the agency of "melanoblasts," or even any aid from the cutis. There are both epidermal and cutis melanoblasts, and they are independent of each other. Pigmented epidermal cells are capable, under certain stimuli, to assume irregularly branching forms (filling the intercellular spaces) simulating mesodermal chromatophores (so-called melanoblasts) which might have wandered into the epidermis. This is probably the correct interpretation of Karg's figures. He brings forth cytological evidence to show that the pigment arises from a reddish nuclear substance (a "pyroniuroter Kernsubstanz")—he does not commit himself as to its chemical nature—which passes into the cytoplasm and gradually assumes the yellowish

brown color of the melanic granules. These results from experimental procedure are confirmed by findings in the retina of the calf embryo, where the identical stages in pigment formation are observed. He thinks it more probable that the pigment has origin in an albuminous substance (this is in complete accord with the work of Chittenden and Albro—1903) of the nucleus than that haemoglobin has any contributory rôle. The earlier stages in such a process can be observed in carcinoma without progress to the final stage of pigmentation. This indicates that we are probably dealing with a nuclear substance, which, shed into the cytoplasm, under the influence of an oxidation enzyme, becomes a melanic substance.

As bearing on the point of the origin of pigment my own observations are as follows: No undoubted branching pigmented cells can be seen among the colored epidermal cells in any of my specimens. Occasionally a process of a pigmented connective tissue cell of the cutis is seen to extend for some distance into the rete mucosum Malpighi (Fig. 1). But their number seems very much too meager to supply the pigment of the many colored cells of the epidermis. There is a nice correspondence between the relative abundance of pigmented cells in the dermis and epidermis of the several specimens of skin. In light skins there are few of each type in each layer; in darker skins there is a decided increase in both. But this proportional increase is as reasonably interpreted as due to the same cause influencing both layers, as that the increased number of pigmented epidermal cells demands an increased number of cutis melanoblasts. Moreover, when one considers that there is a continual exfoliation of the superficial layers and a replacing of the same from the lower layers, the number of epidermal pigmented cells in colored skins seems out of all proportion to the number of the cutis melanoblasts which are supposed to furnish the pigment.

The pigment cells of the cutis are most abundant along the border between dermis and epidermis and along the capillaries of the vascular papillæ. This first point would seem to indicate the function ascribed to them by Karg

and others, but when one assumes (as all investigators agree in doing) that pigment formation is somehow related to the blood as source of nutrition or supply, the segregation of the pigmented cutis cells at this level becomes intelligible on other grounds, *i. e.*, necessary closeness to the capillaries of papillæ. Only those layers of the epidermis next the border line (*i. e.*, next the capillaries) have the pigment granules of the prevailing size and color for normal pigment cells. Thus my evidence points to a dependence of both cutis and epidermal cells upon the same source (the blood of the capillaries) for a *sine qua non* of pigment formation, and an independence of each with respect to the other as a necessary source of supply or even as an aid to pigmentation. That the blood constituent is not hæmoglobin the arguments of Karg and the observations of Meirowsky seem conclusively to prove. That it is not an iron-containing element (*e. g.*, hæmosiderin) I have demonstrated by the method of testing with potassium ferricyanide as used by Brown (1910) for the liver. Chemical analysis by Abel and Davis (1896) also has shown that the melanic pigment of the hair and skin of the negro is free of iron. The evidence at hand, as furnished by Chittenden and Albro, von Fürth, Spiegler, Gessard, Riddle, Meirowsky, and others seems to render it very probable that in vital melanogenesis we are dealing with a proteid substance (tyrosin; trophoplast; chromogen) acted upon by an enzyme or oxidase (tyrosinase) and that one (probably the former) is supplied by the cell (nucleus) and the other by the blood.

The fact that the pigment granules, in epidermal cells that are not packed with them, are segregated in the distal portion, indicates that they are responsive to the influence of light. However, the further discussion will not be complicated by a consideration of this possible factor. The following discussion will accept as well supported the position that pigment is formed in the epidermal cells—the analogous formation of pigment in ganglion cells gives further support—by virtue of a cellular metab-

olism made possible by close association with the nutritive source, *i. e.*, blood vessels.¹

¹ A specimen of leucoderma from a dark negro obtained at autopsy through the kindness of Dr. W. Thalhimer gives further evidence in support of this position. Macroscopically, the two leucodermic areas, about 5 cm. in diameter, and bilaterally symmetrically placed over the clavicles, appeared perfectly normal except for their very much lighter color (grayish yellow). A section through the transition area shows the following histologic conditions: (1) The leucodermic area contains a comparatively very large number of pigmented cells in the corium. (2) In the epidermis the comparatively small amount of pigment present is confined to some of the columnar cells of the stratum Malpighi. (3) The corium of the pigmented skin has only a moderate amount of pigmented cells. (4) In the epidermis of the normal skin, the columnar cells are laden with melanic granules, and all of the more superficial layers of cells contain a considerable amount of melanic pigment. (5) The pigment granules are everywhere the same in respect to color and size. (6) The epidermal cells appear identical, except for the variable abundance of the pigment granules, in the two regions. In view of the above facts, it seems clear that lack of pigment in the epidermis of the leucodermic patch is not due to a dermal deficiency (supposing the derma to be its source of supply), nor to an inability on the part of the epidermal cells to take up pigment (since they harbor a small amount). These facts speak in favor of the epidermal origin of the epidermal pigment, and indicate a local inability on the part of the epidermal cells to manufacture in normal quantity (for this individual) the melanic granules. Furthermore, interpreting melanogenesis in terms of tyrosin and tyrosinase, the facts indicate a local deficiency or inhibition of one of these factors. It seems more reasonable to suppose that the columnar cells of the epidermis of the leucodermic areas were for some obscure cause (nervous?) unable to elaborate the granules ("pyroninroter Kernsubstanz," Meirowsky) which, under the influence of an oxidase probably everywhere present, turn melanic.

The recent work of Dyson ("An Investigation on Cutaneous Pigmentation in Normal and Pathological Conditions," *Journ. Path. and Bact.*, 15: 3, 1911) in the main also confirms Meirowsky's findings regarding the nuclear origin of the melanic granules in the epidermis of pigmented skins (after treatment with the Finsen lamp). Dyson describes "blue granules which owe their staining capacity (in haematoxylin) after bichromating to the presence of an unsaturated fatty substance," very similar to the "pyronin-red substance" of Meirowsky. These granules Dyson regards as "the mother substance of pigment. These granules are complex in character, being composed of a lipoid and proteid element; the proteid portion precipitates out after its escape from the nucleus and remains as a foreign body in the cytoplasm of the cell; whilst the larger granules seen at the periphery of the cells I regard as the lipoid substance after the separation has taken place; these granules then escape into the intercellular lymph spaces" (p. 314). The "pyronin-red" substance he interprets as "probably the chromatic proteid portion of the complex granules from which the lipoid portion has been dissolved by his (Meirowsky) method of preparing his material," *i. e.*, use of alcohol (p. 316). In origin melanotic pigment is then a lipochrome, the melanin being the chromatic proteid portion after

Cause of Degree of Coloration.—Theoretically at least six possibilities are conceivable: (1) Number of pigmented epidermal cells, or, indirectly, (2) number of pigmented connective tissue cells of cutis, (3) number of pigment granules in pigmented epidermal cell, (4) coloration of granules in pigmented epidermal cells, (5) numbers 1 and 3, (6) combination of 1 or 3 and 4, or a combination of the three. It is also theoretically possible that a difference in the size of the granules may play a part in determining the degree of coloration. In view of what was said under the previous heading, number 2 can be disregarded and there remain five plausible possibilities. Which "possibility" or set of possibilities expresses the reality will become clear from the description of the several types of skin. In anticipation of ensuing results it may be said in brief that there seems to be only one factor in skin coloration, viz., the number of the pigment granules, a greater number of granules of course involving a greater number of cells—with a small and possibly negligible variation in size.

DESCRIPTION OF NEGRO SKINS

The several samples of skin will be described in the order of their degree of coloration as seen from the surface. This agrees almost absolutely with the degree (*i. e.*, quantity) of the pigmentation factor, as will appear below.

its separation from the complex lipoid granules. In several points my findings in leucoderma do not accord with those of Dyson: (1) While it may be true that the pigment granules of the cutis cells are slightly coarser, I can not regard them as darker than those of the epidermis. (2) In no case have I been able to discern melanic granules in the nuclei (studied in *unstained* preparations). (3) I find a greater relative amount of cutis pigment in the specimen of leucoderma, whereas Dyson reports no pigment in the cutis of his two samples of leucoderma. (4) Consequently (and for still other reasons) I can not accept his position that cutis pigment is secondary to epithelial, *i. e.*, that pigment passes from epidermis via lymph channels to the derma where it is supposed to be taken up by wandering cells. If this were true the cutis underlying the more highly pigmented epithelium should contain relatively (to leucodermic area) more pigment. But just the reverse is the case in my specimen. In fact, the amount of pigment in the epidermis would seem to be too meager to supply the cutis pigment present under the leucodermic patch. All the evidence indicates that the path of pigment transportation is from depth to surface of epidermis.

All the samples being taken from the same body region, there is a very close correspondence in the number of epidermal layers involved.

Number 9: Here all the epidermal strata of cells are pigmented. In the basal layer the cells are packed with the yellowish brown spherical granules to such an extent as partially to obscure the nucleus and cause the cell to bulge. In the more superficial layers, the granules are massed distally and more scattered proximally, the nucleus appearing very prominently. In the cutis, chromatophores are abundant, their granules being similar in shape, size and color to those of the basal epidermal layer. These cells are always in the near vicinity of the blood vessels. In the upper layers of the epidermis the pigment granules become darker, finer and frequently of oblong shape. These several differential features are ascribed to the several factors of desiccation, pressure and keratization that the containing cells undergo in their passage to the exfoliating surface. The same explanation probably (at least to some degree) applies to the invariably darker bacillary pigment granules of the shaft of the hairs appearing in all of the sections of this first group.² In sections of child's scalp, however, both shaft and bulb contain the same yellowish-brown granules as found in the skin, the only appreciable difference being a considerable irregularity in shape and size. It must be noted here that not all the basal cells (though in negro skin the exceptions both in dermis and epidermis are rare) are equally packed with granules. The optical effect of a small number of granules is a lighter shade of brown than that given by a denser mass of identically colored granules.

The objection may be raised that discrimination or lack of discrimination between a darker and lighter pigmented granule is the result of an interpretation where the "personal equation" may factor largely. It is not denied that it is difficult to satisfy oneself absolutely that the granules

² I have recently found a very striking exception to the usual brown color of melanin pigment under ordinary conditions in certain cells of young turtles (ca. 25 mm. stage of development). Here growing and dividing cells of the choroid, epidermis, connective tissue generally, and bone marrow contain absolutely black pigment granules.

of the various basal cells of the same and different samples of skin are of identically the same color (the color difference between the skin pigment granules and those of the sections of attached hair is decided enough) but all possible caution was observed to offset the personal factor. In short, when the writer after much study was still somewhat undecided as to a definite stand, the slides were shown to three different professors, all with long training and much experience in the use of the microscope. These men were asked to answer the following questions with respect to the basal cells of the eighteen samples, and this without knowledge of what the other men had written: Are the pigment granules of the samples of skin of the same color or of different colors? Name the color or colors? The replies were uniform in recognizing only one type of granules, and in describing it as "yellowish brown," "brownish yellow" and "a dark golden or yellowish brown—amber—somewhere between brass and copper," respectively. One man noted the slightly darker shade of the granules of the more superficial cells.

Number 14 (Fig. 1): All the layers are again pigmented, but there is a slightly smaller amount in the upper layers than in number 9. The pigment cells of the cutis are here somewhat more numerous, showing many anastomosing processes, and forming in places a network of pigmented strands just beneath the scarf skin. A variation in amount of pigment in different regions of the basal layer is again evident. The deeper color of number 9 as compared with 14 seems due to the greater amount of pigment present in the superficial layers.

Numbers 1 and 2 are very like the foregoing except that there is a slight decrease in the number of greatly packed basal cells.

Number 15: This sample shows a quite appreciably smaller number of pigment cells in the epidermis and a yet more pronounced decrease in the corium.

DESCRIPTION OF MULATTO SKINS

Number 4: The number of granules in the great majority of the basal cells of the epidermis is somewhat less

than in the last of the foregoing group. Cells also now appear in the lower layers with only relatively scattered granules. There is an almost complete absence of granules from the cells of the superficial layers. There are decidedly fewer pigmented cutis cells. Hair in section again shows the same sort of pigment granules as above described. This statement holds good for hair wherever they have appeared in these sections (not seen in the samples of blond skin).

Number 3 (Fig. 2): The pigmented basal cells of this specimen contain still fewer granules than in number 4. The very small number and size of the chromatophores of the corium is striking. Here again only the cells of the rete mucosum Malpighi contain the granules.

Number 8 is almost identical with number 3.

Number 17 is like number 8 with slightly less pigment in the rete mucosum Malpighi; but here the superficial layers again contain a considerable quantity of melanin granules and the melanic cutis cells are more abundant than in numbers 3 and 18.

Numbers 10 and 18 are both very like number 8, showing only a slight decrease in the quantity of pigment.

This seems the best place for a brief discussion of the foregoing facts. What is the fundamental cause of the difference in the degree of color of the skins described? Plainly, I believe, a difference in the abundance of the pigment granules in the basal cells of the rete mucosum Malpighi. A densely packed mother cell of this layer gives rise to two daughter cells of very similar constitution which are only slightly altered as they pass to the upper layers. Hence in skins where the basal cells manufacture much pigment, the entire rete mucosum Malpighi, formed of its descendants—the factors of desiccation and cornification not being able at once to produce a very appreciable destruction or modification of the granules—will appear pigmented. But between negro and mulatto skin there does not seem to be any apparent difference as to the number of epidermal cells producing pigment, but only as to the quantity of pigment produced by the same basal cells, an initial greater quantity determining a secondarily persisting greater quantity, and thus an apparently greater num-

ber of epidermal pigmented cells. Probably also, the cells generally of the rete mucosum Malpighi retain in small degree the property of the basal mother cell to produce pigment granules.

DESCRIPTION OF SKIN OF BRUNET

Number 6 (pathologically pigmented skin) : Here one is unable to determine just how much pigment is due to the normal ("midway between blond and brunet") and how much to the pathologic condition ("thyroid adenomata" — "gall stones, but not jaundiced"). The specimen contains fewer granules than number 18 and more than number 7.

Number 7: Here the granules are very few in number and confined exclusively to the basal layer. There is great variation in the number of granules held by the basal cells. Only very rarely does a small pigmented cutis cell appear in the sections.

Number 11 has still fewer granules than 7 and

Number 17 shows only occasional cells of the basal layer slightly pigmented (few melanin granules).

Number 16 is more like the blond skin to be described. There are very few granules in only occasional cells; and no pigmented cutis cells are seen in the section. This skin could not be told from blond.

DESCRIPTION OF BLOND SKIN

Number 13 (Fig. 3): Here the layer of distinctly pigmented basal cells is fairly complete. The section is very like number 7 of the brunet series.

Number 5 is almost identical with number 16. The pigment granules are very rare; and only a few to a cell.

It must be emphasized at this point that the melanic granules of number 5 are in point of shape, size and coloration indistinguishable from those of number 9.

There is a continuous gradation in color (and the number of pigment granules) from negro to blond skin with an overlapping at the extremes.

The melanic granules of the specimen of melanotic sarcoma were of the same yellowish-brown color (perhaps of a trifle lighter shade), but of very irregular shape and

with great variations in size (the larger "granules" may be the result of fusion of smaller masses).

STATEMENT OF RESULTS AND RELATED FACTS

The facts whose interpretation is sought in terms of some principle of heredity are these: (1) The degree of skin coloration is due to the variable number of pigment granules in the cells of the rete mucosum Malpighi involving incidentally a variable number of more superficial cells. (2) The pigment granules (melanic) of all skin (albinos excepted) are identical in size (practically), shape and color (without qualification). (3) The ascending scale of morphological conditions paralleling a progressively deepening grade of pigmentation may be described as follows: (a) few cells of basal layer pigmented with few granules—blonds, (b) more cells containing more granules—brunets, (c) a more or less complete basal layer of cells with many and very many melanic granules (mulattos), (d) the cells of basal layer packed and distended with pigment granules; the cells of the more superficial layer also with very many granules.

Or, restated and explained, (4) The progressive increase in progressively darker skins both in the number of granules and in the number of the pigmented cells. That these two facts are related to each other as cause (number of granules) and effect (number of pigment cells) is strongly indicated by the fact that in light mulatto and brunet skins, where only the basal cells are distinctly pigmented, the number of granules per cell in general decreases with the progressively lighter shades.

(5) The results recently published by the Davenports showing a segregation of the original skin colors (grand-parental colors) among the individuals of the third generation, *i. e.*, children of mulatto parents.

(6) The accumulation of the pigmented cells near the border line between the dermis and epidermis or in the vicinity of the blood stream.

(7) Melanin formation is an intracellular metabolic process going on independently and in a measure proportionately in both dermis and epidermis. This seems demonstrated by the researches of Meirowsky and others,

and the view is indirectly supported by the comparative findings above described, viz.: (a) absence of connective tissue cells (chromatophores) among the epidermal cells (admitted by Karg for normal negro skin), (b) comparative rarity of pigmented processes from cutis cells, and (c) an apparently disproportionate number of chromatophores as compared with the epidermal pigment cells.

(8) The agreement between the cytologic facts of Meirowsky and the chemical results of Chittenden and Albro, and others, that the antecedent of melanin is some form of proteid.

(9) The observations of Meirowsky showing a passage of granules from the nucleus to the cytoplasm as the initial step in melanogenesis, and a progressive coloration of these granules to a final stage of yellowish-brown pigment.

(10) The production of artificial melanins ("melanoidins"—Schmiedeberg) by Chittenden and Albro and others from "antialbumid" and various proteids, and the results described by Spiegler and Riddle and others indicating an interaction of a chromogen (tyrosin compound) with an oxidizing enzyme (tyrosinase) in the formation of melanin.

DISCUSSION

There appears, then, proximately to be only one factor in skin-pigmentation, viz., the number of granules of identical shade (yellowish-brown), incidentally the number of cells containing such granules. The granules would seem to be the result of intracellular activities (Meirowsky) and to have origin in cell proteids (Chittenden and Albro, and Meirowsky). In terms of chromogen and oxidase, the granules may be thought of as tyrosin which under the influence of tyrosinase from the blood or tissues generally becomes melanic.

The melanogenetic process seems to stop at the same point in all grades of colored skin, from negro to blond—in hair of the same it may possibly proceed slightly further, though even this seems doubtful in view of appearances in the hair bulb. One seems to be dealing, then, with a continuous process, *i. e.*, the production of melanic

granules; and the numerical point at which the process stops determines the color of the skin. But thought of in terms of greater and lesser ability for tyrosin production (intervention of a tyrosinase-producing factor would modify the results, but not fundamentally alter the mechanism of inheritance); and attributing such factor to a specific cell-organ which may be a chromosome or part of such ("teleomorph"—Spillman), the segregation noted by the Davenports becomes as intelligible as other Mendelian phenomena. From the standpoint of the number of granules some mulatto skins are certainly different from a "blend" between negro and white, and this is true in the direction both of more and of less—from the standpoint of the amount of pigment some mulattos are identical, on the one hand, with negros and on the other with brunets—and evidences a measure of segregation of "strong melanogenesis" and "slight melanogenesis."

A plausible interpretation of Karg's experimental findings might be made on the basis of a larger and smaller amount of tyrosinase in negro and white blood, respectively—or more likely perhaps on the basis of more and less pronounced stimulation by negro and white blood, respectively, to tyrosin production.

The occurrence of melanotic sarcomata in albinos and white horses forces the assumption that in both cases tyrosinase is present, as in ordinary colored animals. In albinos there is evidently an absence generally of tyrosin in usual events (production of tyrosin locally accords better with our present knowledge than a hypothesis of local tyrosinase production). If Spiegler's view represents the veritable condition, viz., that in white horses there is present a white melanin—rendered quite doubtful by Gortner's recent work—the end-result of an oxidation process of tyrosin, the presence of melanotic tumors in white horses may be explained in the same way as in ordinary cases, as shown by the work of Gessard.

Accordingly, when one considers the question of color inheritance among crosses between ordinary white individuals and albinos, two factors (at least—these most prominent and apparently most important; a "multiplicity of units" or factors may be involved in color-in-

heritance as the Davenports suggest) appear to be involved, *i. e.*, a tyrosin-producing factor and a tyrosinase-producing factor, one at least a function of the epidermal cell, and both having as likely a chromosomal representative (a "teleomorph"—Spillman) as any other cell organ or function. The tyrosin-producing factor is probably generally absent in albinos, locally appearing abnormally in tumor cells, hence two albinos can never produce colored offspring, as amply shown by the results of the experimental breeders.

The observations of Stedman, reported by Bateson (p. 227) "to the effect that an albino negress married to a European had children all mulattos" does not necessarily imply that the factor determining the blackness of the negro (tyrosin production) was carried by the albino. Mulattos are frequently so classified on the basis of marks other than color of skin. Many mulattos are no darker than many white brunets. In the above case the factor controlling tyrosin production may very well have been contributed by the father alone. This instance does not necessarily controvert the assumption that albinos lack the factor of tyrosin production.

Moreover, crosses between albinos and pigmented individuals result in families where albinism greatly preponderates, as shown in the recent "dissertation" by Stainer. In crosses between whites and blacks one deals apparently more especially with the factors of great and small capacity for tyrosin production—tyrosinase being probably of more general distribution. Judging from the pedigrees published by Stainer, absence of capacity for tyrosin production (albinism) in man behaves more like a dominant character (or at any rate, not like a pure recessive) to the presence of such capacity. This is not in accord with the results of the Davenports, which seem to indicate that "internal conditions that lead to deeper pigmentation dominate over the weaker conditions"; similarly as regards color of hair and eyes, "the more pigmented condition tends to be dominant over the less pigmented" according to the earlier investigations of Holmes and Loomis as well as the more recent work of the Davenports. Nor does it accord with the results of

the experimental breeders with lower mammals. It may, of course, be found that all mammalian albinos have the white pigment (melanin) described by Spiegler for white horses. Such a result would seem to correlate a number of apparently discordant facts. It would obviate the further assumption of an "antioxidase" suggested by Gortner, and render more intelligible the non-recessive behavior of human albinism.³ No theory of color-inheritance is satisfactory that can not embrace all the facts of albinism, and such is the present state of affairs.

In crosses between whites and negroes there is generally a partial dominance of the deeper pigmented condition over the lighter in the second (mulatto) generation; the third generation showing a measure of segregation of the original colors. The partiality and incompleteness of dominance and segregation may be due to a "myriad" other factors modifying and obscuring more or less the final results.⁴

Seeing that we are dealing with only one kind of colored granules, the apparent segregation noted in the families of mulatto parents does not here seem to be due to a condition of unstable equilibrium in the chemical constitutions of the parental melanin and an attempt at readjustment to an original state of greater stability, as suggested by Riddle.

The apparent continuity of the melanogenetic process, as seen in the continuous numerical gradation of the same colored pigment granules where a graded series of skins is examined, rests, in fact, where single families of mulattos are considered, upon discontinuities or discrete

³In the second part of Davenport's paper on "Heredity of Skin Pigmentation in Man" (AM. NAT., Vol. 44, No. 528) is presented an array of facts, drawn from a study of a large number of albino families, that furnishes the most cogent argument yet offered for the recessive nature of albinism.

⁴Professor L. W. Lyde, in an article on "Climate and Racial Skin Color" (*Contemporary Review*, February, 1911), states his conclusion that "pigment is latent in all humans and depends for its development on relative action of the lungs and intestines, which is in turn decided by sunlight and humidity." On the other hand, Professor J. H. F. Kohlrouge ("The Influence of a Tropical Climate on Europeans," *Eugenics Review*, April, 1911) believes that "there is no reason for assuming that a dark complexion is due to climate."

"unit characters" controlling conditions of a more and a less numerous production of melanic granules, which conditions conform more or less closely to an alternative mode of inheritance.

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A COEFFICIENT OF INDIVIDUAL PREPOTENCY FOR STUDENTS OF HEREDITY

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I. THE CONCEPTION OF PREPOTENCY

THE term prepotency conveys the general idea that certain individuals "are particularly apt to impress their personal characters upon their offspring." Like most terms of general biology it has been applied in several different connections. The much-needed threshing over of the literature to separate the few measures of wheat from the stacks of straw and weeds falls outside the scope of this note.

One may follow Vilmorin, Hallett, Hays and many other noted breeders in the recognition of the practical importance of the fact that two individuals may be externally exactly alike and yet produce quite dissimilar offspring, without pledging himself to any of the theories of heredity in support of which it is sometimes cited. The aim of the practical breeder is not to formulate or to test theories of heredity but to get a strain of wheat which will draw the maximum amount of flour from an acre of soil or a breed of beasts that will yield the largest net dividends in milk, eggs or steak. His problem is pre-eminently a practical one, and one of the greatest services the student of biology can render him is to provide the criteria which enable him to select as easily as possible the parents of a race which will meet his requirements.

The purpose of this note is to call the attention of students of heredity to certain formulæ¹ which may be of

¹ These formulæ have heretofore been used in anthropometric surveys in testing the divergence of the inhabitants of a restricted community from the population of the whole area under consideration. They are equally

service in estimating the desirability of individual parents.

By individual prepotency² we understand for present purposes the phenomenon of certain individuals, or pairs of individuals in bi-parental inheritance, being exceptional in their capacity for producing offspring of any given characteristic.

As used here the term prepotency is most general. It implies nothing concerning the somatic similarity³ of parent and offspring and is in no way dependent upon any theory of heredity. It merely expresses a fact well known to practical breeders for half a century.

By a coefficient of individual prepotency one understands a statistical constant which measures the degree of superiority (with respect to the capacity for the production of offspring of any desired type) of any single parent or pair of parents.

well adapted to determine the significance of the deviation of an individual family from its generation.

The fundamental papers are:

Pearson, K., "On some Properties of the Hypergeometrical Series, and on the Fitting of such Series to Observation Polygons in the Theory of Chance," *Phil. Mag.*, February, 1899, p. 239.

Pearson, K., "On the Curves which are most Suitable for Describing the Frequency of Random Samples of a Population," *Biometrika*, Vol. V, pp. 172-175, 1906.

Pearson, K., "Note on the Significant or Non-significant Character of a Sub-sample drawn from a Sample," *Biometrika*, Vol. V, pp. 181-183, 1906.

Pearson, K., "On a Coefficient of Class Heterogeneity or Divergence," *Biometrika*, Vol. V, pp. 198-203, 1906.

Tocher, J. F., "The Anthropometric Characteristics of the Inmates of Asylums in Scotland," *Biometrika*, Vol. V, pp. 315-318, 1907.

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²The disadvantages of using a word which has been so variously employed as prepotency are offset by keeping the terminology as simple as possible. The important thing is to have all terms carefully defined and unambiguous wherever used.

³In any study of heredity the correlation between the somatic characters of the parents and their offspring must be taken into account. Prepotency may, however, be estimated solely from the germinal characters of the parents as expressed in their respective arrays of offspring.

II. THE MEASUREMENT OF INDIVIDUAL PREPOTENCY

However well one may know the somatic characters of an individual or however intimate his knowledge of its ancestry the ultimate test of its value as a starting point for a new race is the quality of its offspring. *The proof of the parent is its produce* has been recognized as valid by various breeders since the time of Louis Vilmorin, who separated the parent beets and judged them individually by their offspring. The "ear-to-row" test in corn breeding, Petkus von Lochow's row-tests in rye and Hays's "centgener power" all represent attempts by practical breeders to obtain measures of individual prepotency as the term is used here. Galton's study of the distribution of prepotency in horses falls in the same class.

The method of estimating prepotency directly from the mean value (e. g., sugar content) of the offspring, or from the number of offspring surpassing a given standard (e. g., a mile in 2:30, or better, on the track) has disadvantages which will be obvious to those acquainted with elementary statistics.

So far as I am aware the credit of first recognizing the need of taking into account both type and variability in the criterion by which the relative desirability of the individual parents should be judged is due to Waugh.

In discussing some results secured on experiments with peas he remarks:⁴

... There were, as always, some exceptional cases of individual vines which showed a marked ability to transmit their individual characters to their offspring. The selection of such prepotent plants is evidently an important matter in plant breeding. In order to exhibit this difference we have computed a coefficient of heredity for each parent and for each character under study.

Waugh's formula is

$$C = 1/\sigma D,$$

where

C = coefficient of individual heredity,

σ = standard deviation of offspring,

⁴ Ann. Rept. Mass. Ag. Exp. Sta., Vol. 21, p. 172, 1909.

D = difference between numerical value of the parent character and the mean of the same character in the offspring.

In a later report he makes use of this formula "in an attempt to answer the question whether prepotency is inherited or not."⁵

Now while Waugh deserves all credit for suggesting the need of a coefficient of individual prepotency, I think the formula he proposes can not be justified theoretically nor regarded as practically satisfactory.⁶

The requirements of a coefficient of individual prepotency are at least the following:

(a) The comparison must be made between the offspring families, not between the individual parent and its offspring.

(b) The comparison must be so drawn as to attach importance only to differences significantly greater than the probable errors of random sampling.

(c) The coefficient expressing prepotency should be relative, *i. e.*, it should be comparable from character to character.

Proposition (b) and (c) will be granted without argument. In justification of (a) it is only necessary to point out that from the standpoint of the man who wishes to decide which families to continue to propagate and which to burn, the ideal method is one which may be applied to the individuals of any one generation entirely independently of those of any other. Of course this is not to be interpreted as a recommendation that in the routine work of practical or experimental breeding only one generation should be considered. What is meant is that it is desirable to have formulæ which permit of a consideration of prepotency on the data of any (offspring) generation independently. Such a formula does not preclude or render inadvisable the study of many ascendant generations.

⁵ Waugh, *loc. cit.*, Vol. 22, pp. 172-175, 1910.

⁶ Indeed he himself has pointed out some of the difficulties and has suggested that a better formula might be found.

The necessity of dealing with each generation independently is also imposed by the possibility of a differentiation between any two generations due to purely environmental (meteorological or edaphic) influences. Taken as a whole the entire offspring generation may be superior or inferior to the parental generation; and this because of no hereditary influence of the parents at all, for all families may be raised or lowered proportionally. This fact vitiates at once any comparison between individual parents and individual offspring unless the type and variability of both parental and offspring generation are taken into account.

In the practical work of calculation two cases may be conveniently recognized: in the first, the character is capable of direct measurement on a quantitative scale, in the second, the character is not capable of direct measurement but the individuals may be grouped into satisfactorily defined classes. In the first case the means may be compared; in the second case the proportional frequencies of one class must be used.

(a) *Case of Characters Measurable on a Quantitative Scale*

It is well known that the standard deviation of a mean is σ/\sqrt{N} and its probable error is $.67449 \sigma/\sqrt{N}$. Given two uncorrelated means m and M , their difference and its probable error is given by

$$m - M \pm .67449 \sqrt{\frac{\sigma^2}{n} + \frac{\Sigma^2}{N}},$$

where σ and Σ and n and N represent the standard deviations and the numbers of individuals involved in the series. Thus it is quite easy to test the significance of differences in means between any two samples, or families in our case. But with a large number of families the labor of a series of such comparisons is prohibitive. What we need is some easily calculated criterion of the biological significance of the deviation of the mean of an

individual family from the mean of the population to which it belongs.

Suppose a population composed of N individuals with a mean of M and a variability of Σ is due to P parents. Now if this population be divided into two random samples of n and N' individuals, m and M' means, and σ and Σ' variabilities the differences in their means will be

$$(m - M') \pm .67449 \sqrt{\frac{\sigma^2}{n} + \frac{\Sigma'^2}{N'}}$$

But Pearson has shown that the difference between the mean of a sub-sample m which in our case may represent the offspring of a single parent (or pair of parents) and the population mean M is not given by the preceding formula since n is included in N . The formula for such a case as this he has shown to be

$$(m - M) \pm .67449 \sqrt{\frac{\Sigma^2}{N} + \frac{\sigma^2}{n} \left(1 - \frac{2n}{N}\right) - \frac{n(M - m)^2}{N(N - n)}}$$

This is the formula which we are seeking, the probable error of the difference between the mean for any family and that for the whole population. By calculating $(m - M)/E_{(m-M)}$ for every family we should have a criterion of its superiority or inferiority—the individual prepotency of the parent in question—relative to the average condition in the series to which it belongs.

Tocher has pointed out advantages in using $(m - M)/\sigma_{(m-M)}$ instead of $(m - M)/E_{(m-M)}$, but this is merely a matter of convenience. The significance of the ratios can be tested by tables of the normal curve.

(b) *Case of Characters not Measurable on a Quantitative Scale*

For characters not quantitatively measurable two methods of treatment are available. The first consists in testing the divergence of a family from the general population on the basis of the relative frequency of a given character. The second consists in testing the deviation

of a family from the population with respect to the distribution of a character. At present the second of these methods seems of little practical importance for our purpose because of the relatively small numbers of individuals available in breeding experiments, even with plants, and because of the arithmetical routine.

Consider the first method. Let N be the number of individuals in a population due to P parents. Let X be a character common to all but appearing in different intensities (say from 0 development to the greatest possible intensity) in the several individuals, not measurable but capable of division into m classes. Let $s_1, s_2, s_3 \dots s_m$ be the classes and $y_{s_1}, y_{s_2}, y_{s_3} \dots y_{s_m}$ be the frequencies in the population as a whole. Now if a single family of n members be observed the probability of an individual belonging to any class, say s_2 , is $y_{s_2}/N = p$, while the probability of its not belonging to that class is $(1 - p) = q$. The actual number of individuals with character s_2 in the family should be $np = y'_{s_2}$, while the frequency for the $m - 1$ remaining classes within the family will be given by $y'_{s_1}, y'_{s_3}, y'_{s_4} \dots y'_{s_m}$ providing (a) that the family is not differentiated from the population, *e. g.*, that there is no individual prepotency in the sense that we have used the term, and (b) that n is so large that the probable errors of random sampling are negligible. In actual work (b) can never, or almost never, be realized. Our problem is to determine whether differences between the theoretical class frequencies, y'_s , and the actually observed class frequencies, y''_s , in the family are to be regarded as due to chance merely or whether they are so large that they can reasonably be considered as indicating a differentiation of the family from the population to which it belongs. In short, our problem is to test $(y''_{s_2} - y'_{s_2})$ against its probable error.

Pearson has shown that the standard deviation of $(y''_s - y'_s)$ for any grade is

$$\Sigma(y''_s - y'_s) = \sqrt{npq \left(1 - \frac{n-1}{N-1} \right)}$$

and Tocher has pointed out that as a test for significance of divergence we may use either of the three ratios

$$(a) \quad (y_s'' - y_s') / \sqrt{npq(N-n)/(N-1)}.$$

$$(b) \quad 100\{(y_s''/n) - p\} / \sqrt{100^2 pq(N-n)/n(N-1)}.$$

$$(c) \quad 100\{(y_s''/y_s') - 1\} / 100\sqrt{q(N-n)/np(N-1)}.$$

The significance of these ratios can be judged from the tables of the probability integral.⁷

III. RECAPITULATION

Individual prepotency is here used to designate the superior capacity of certain parents for producing offspring of any desired character. The conception is most general, and does not imply a similarity in soma between parent and offspring, but the prepotency of the parent is judged entirely by the offspring it produces. The term is used merely to describe a long-known phenomenon, and no theoretical explanation is suggested.

Various breeders have tried to obtain a measure of individual prepotency in its present significance. The purpose of the present note is to point out certain biometric formulæ, in use for other purposes for several years, which seem well adapted for this purpose. They at least obviate several of the objectional features of some of the methods which have been employed. Their applicability in practical work will probably be limited by the arithmetical routine, but in experimental studies their importance may be very considerable. Illustration of their application will be published soon.

COLD SPRING HARBOR, N. Y.,
May 19, 1911.

⁷ Of course a statistical formula is not applicable to cases not covered by the assumptions on which it was developed. It seems unnecessary to discuss these here. Those using the formulæ should familiarize themselves with the limitations laid down by Pearson and Tocher in proposing the formulæ.

THE ADAPTATIONS OF THE PRIMATES

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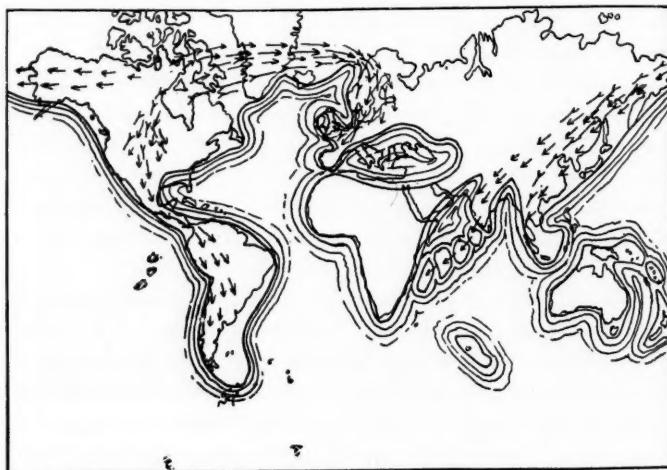
THE development of the primates has taken place in regions of comparatively high temperatures, especially in tropical and semitropical climate. This is chiefly due to their arboreal adaptation, which keeps them where the trees throughout the year offer food either as fruit, leaves, blossoms, insects or small animals.

The first primates are yet to be found, but they doubtless lived either during the last of the Cretaceous or in the earliest Eocene; for during the Lower Eocene of the Wasatch epoch there suddenly appear in America two well-distinguished families of primates, the general feeders or *Notharctidæ*, and the fruit eaters or *Anaptomorphidae*.¹ Between these no intermediate or ancestral group is known, but the wide divergence in form would indicate a considerable time element for development. The genera *Anaptomorphus* and *Pelycodus* appear in America as a part of the wave of migration which introduces for the first time representatives of the modern groups of mammals. Somewhat later the primates appear in England and France, apparently part of the same original stock but differing slightly as a result of independent development.

The original group of primates or ancestral stock seems to have been a large-brained arboreal insectivore, somewhat similar to the tree shrews (*Tupaiidæ*). Apparently their home was to the north in the Hudson Bay

¹The considerable group termed Proglires by Osborn and including *Mixodectes*, *Microsyops*, *Cynodontomys*, *Indrodon*, *Olbodotes*, etc., all having in common the gnawing adaptation and a very primate-like set of premolars and molars, are now assigned by Matthew and Osborn to the Insectivora.

region or further north in the forest areas; and under the decidedly tropical climate which is evidenced by the palms and ferns, crocodiles and primates themselves.² From this ancestral center the first primates, along with other groups, migrated in all directions possible, climate and land bridges being considered. This opened three paths, one south into America, a second southeasterly into England and France, and a third southwesterly into Asia, thence ever southerly across China and India and along the Indo-Madagascar isthmus (or chain of islands) to Madagascar and Africa.



→ General Feeders

→ Fruit Eaters

FIG. 1. Diagram of the radiation of the primates in the Eocene.

The first primates, as indicated, separate into two groups, first a group of long-headed (dolicocephalic) general feeders with unspecialized teeth, which probably took fruit, leaves, insects and small animals: and second a group of short headed (brachycephalic) fruit-eaters with crowded and rather high pointed teeth. These are

² For a discussion of the climate see Wortman, *Amer. Jour. Sci.*, 1903, Vol. 165, p. 417; and Wieland, same journal, Vol. 166, p. 401, 1903.

the first adaptations of the primates and it probably took some time to arrive at the degree of difference found in the Wasatch of North America.

The fruit-eating brachycephalic group includes *Anaptomorphus* of American Eocene, *Necrolemur* and *Microchærus* of the European Eocene, and *Tarsius*, now living in southeast Asia. During the Eocene the climate was progressively colder, becoming at least temperate by the

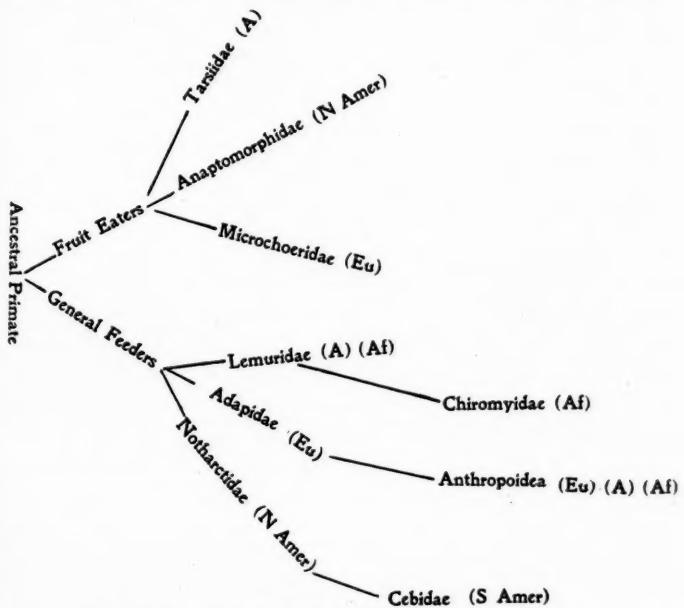


FIG. 2. Genealogical relationships of the Eocene primates.

end of the Eocene in the northern parts of America. This climate acted to force southward all the primates of the north and also several other groups, so that during the Lower Eocene we have the whole primate group pushing down, the Anaptomorphidæ all over North America, the Microchoeridæ on to what there was of Europe and the ancestors of *Tarsius*³ on to eastern Asia,

³ Earle, AMER. NAT., Vol. 31, pp. 569-575 and 680-689, 1897.

which through the lower and middle Eocene was separated from Europe. The fact that *Tarsius* is confined to islands possibly explains why it has remained in so primitive a condition in many ways, though specialized in the limbs which are as yet unknown in any others of this group.

The general feeders are a larger and more abundantly preserved group. It includes the *Notharctidae*⁴ of North America (to which belong *Pelycodus* and *Notharctus*); the *Adapiidae* of Europe⁵ (including *Adapis* and *Plesadapis*); the *Homunculidae*⁶ of South American Miocene (including the genera *Homunculus*, *Pitheculites*, *Homunculites*) and lastly the living lemurs of southern Asia, Madagascar and Africa. All have the dentition $\frac{2}{2}$, $\frac{1}{1}$, $\frac{4}{4}$, $\frac{3}{3} = 40$, and long heads, and apparently ate both vegetable and animal food. The group originated like the foregoing in northern America and migrated southward, driven by the change in climate. The earliest known forms are those in the Wasatch of western America, and they are likewise the most primitive. Though preserved only in Wyoming and New Mexico, they probably occupied pretty much all of our western plains country, then forested. South America seems to have been isolated from early Eocene times, so that some representatives of this group probably got into that continent by early Eocene times, *i. e.*, the radiation over North America must have been pretty rapid and general by lower Eocene times. Those in North America after the separation of South America flourished for some time, being especially abundant in the Wind River and Bridger epochs, but with the cold of the Uinta epoch they were crowded south and finally exterminated in North America, never more to be widely distributed on that continent.

⁴ See Osborn, *Bull. Amer. Museum Nat. Hist.*, Vol. 16, pp. 169-214, 1902, except *Hyopsodontidae*; and Loomis, *Amer. Jour. Sci.*, Vol. 171, pp. 277-285, 1906.

⁵ See Schlosser, "Die Affen, Lemuren, Chiropteren, etc.," des Europäischen Tertiärs, Theil 1, s. 19-54, 1887.

⁶ See Ameghino, *Anal. d. Museo Nac. d. Buenos Aires*, Vol. 15, pp. 424-429, 1906.

Those in South America quickly differentiated by the loss of the first premolar, making a dental formula of $\frac{2}{2}, \frac{1}{1}, \frac{3}{3}, \frac{3}{3} = 36$. With this also goes a deepening of the ramus of the lower jaw, a shortening of the face, and a tendency to develop the occipital region so that it overhangs widely the foramen magnum. This group of forms is termed the *Cebidæ*. On becoming successfully adapted to the South American continent and during the long isolation of that area, these early forms have gradually adapted themselves in various directions, often paralleling old world types. In size they have developed, the largest forms having a body of 27 to 28 inches in length, and legs as long, making a height of $4\frac{1}{2}$ feet, which is a good-sized monkey. They have always remained arboreal with opposable thumbs and a prehensile tail; but they vary from the slender spider monkeys to the robust and powerful woolly monkeys (*Lagothrix*). From the above has been specialized the family of marmosets (*Hapalidæ*), by the loss of the last molar (making the dentition $\frac{2}{2}, \frac{1}{1}, \frac{3}{3}, \frac{2}{2} = 32$), by the development of a broad nasal septum, the loss of the prehensile character of the tail and opposability of the thumb.

The southwesterly wave of migration crossed the Behring's connection and moved down the easterly part of Asia across the Indo-Madagascar isthmus and into Africa. This isthmus or series of islands sank at or toward the end of the Eocene, leaving lemurs stranded all along the area occupied by the isthmus. Those on the islands and especially on Madagascar have remained very much as they were, adapting themselves in minor ways, but being always arboreal. Some peculiarities must have developed very early for they are common to the group, like the having of the lower incisors projecting forward (proclivous), the lower canine small and like an incisor, while the first premolar acts as a canine tooth. Then the fourth digit of the hand is longest, and the second one of the foot is clawed instead of having a nail. In this lemur group we know only the immediate

ancestors of the living forms, and as yet no record has been found of the forms intermediate between those on America and the living types. In the case of the Madagascar form, *Chiromys* or the aye-aye, we have a representative of the group which has adopted a gnawing habit to get grubs, etc., under the bark, and a great change has resulted in the dentition, by which the first incisor has become specialized into a rodent-like gnawing tooth and there is a reduction in the teeth so that the formula is only $\frac{1}{1}, \frac{0}{0}, \frac{1}{0}, \frac{3}{3} = 18$.

The easterly wave of migration is represented by several species of *Adapis* found in the middle and upper Eocene of England and France. Apparently the progress of this easterly migration was slower, so that they reach Europe considerably later than the same latitude in America. The primates are not in the front wave of immigration on the European side, so that it is possible that the forested condition was not as favorable. The Adapiidae in Europe, small primates with a long low skull and the ancestral dental formula $\frac{2}{2}, \frac{1}{1}, \frac{4}{4}, \frac{3}{3} = 40$, the teeth being very generalized.

With the close of the Eocene the first adaptive radiation of the Primates was complete, and they had achieved an almost world-wide distribution. At the end of the period the North American contingent was extinct, the South American group was isolated, the Asiatic and African forms were scattered on islands and on the African continent, and the European contingent was located in central and southern Europe, or what land there was at that time in those regions (see Fig. 3); and it is among these that the next act in the great primate drama took place.

The Oligocene period is one in which there was a gradual rising or emergence of continental areas so that the southern part of Europe was an archipelago, which toward the end developed into a long peninsula, extending from the present Asia Minor (see Fig. 3). During this period the change in the Adapiidae is but little known, but

during that time they shortened the skull and lost the first two premolars, and made a considerable increase in size. In Europe their remains are very scarce and confined to the Lower Oligocene when it was the true *Adapis* which was holding over from the Eocene. Schlosser has just reported some primates from the Fayûm formations of Oligocene age in northern Africa. These he gives new generic names, *Mæripithecus*, *Parapithecus* and *Propliopithecus*, assigning the first two to the Cercopithecidae and the last to the Simiidae. They seem from the descriptions to be primitive members of the Cercopithecidae, which would indicate that the change to the modern type by the loss of the first two premolars was accomplished in the early Oligocene, perhaps in Africa as the two areas are in connection at the time across Gibraltar.⁷

At the beginning of the Miocene the European primates had the dental formula $\frac{1}{2}, \frac{1}{1}, \frac{2}{2}, \frac{3}{3} = 32$, a shortened face, and a shortened tail, but were still arboreal forms. During the Lower Miocene two divisions arise, the one adhering to the quadrupedal gait, the heavy jaws and longer snout: the other acquiring the bipedal gait, and shortening the face with a corresponding broadening of the teeth. In both divisions there is a tendency to come down to the ground.

The former group is the Cercopithecidae in its broad sense, or "old world monkeys"; while the latter are the Simiidae or apes.

The Cercopithecidae seem to run back to some such form as the *Oreopithecus*, found in northern Italy, and presenting dental characteristics resembling the baboon, but at the same time having a shortened face suggesting the Simiidae. A second form belonging to this group is *Mesopithecus* found in considerable abundance in the Lower Pliocene of Greece. This form seems to be intermediate between the macaques and langurs, resem-

⁷ For geography see Matthew, *Bull. Amer. Museum Nat. Hist.*, Vol. 22, p. 364, 1906. For the Fayum Primates, see *Zoologischen Anzeiger*, Bd. 35, for March, 1910, and Matthew, *AMER. NAT.*, Vol. 44, Nov., 1910, p. 700.

bling the former in the stout limbs, the latter in its dentition. Considering the different subfamilies it would appear that the Cercopithecidae originated in southern Europe, that it was fairly successful, and that as a result of this, the family adapted itself in three directions; first one group left the trees and took to life on the ground, giving rise to *Cynocephalus* and *Macacus*; the second group became leaf feeders, and developed a pouched stomach and for some reason also disproportionately long hind limbs, giving rise to *Semnopithecus* and *Nasalis* of Asia and *Colobus* of Africa: while those remaining in the trees and changing but little are *Cercopithecus* and *Cercocebus* of Africa.

The differentiations took place in the Miocene and are fundamentally based on food supplies. Those forms which had developed strength enough to defend themselves, their fore and hind limbs being approximately equal in length, and their food including insects, lizards, frogs, etc., as well as all sorts of vegetable life, like leaves, fruit, blossoms, etc., came down from the trees.

The terrestrial forms which continued to live in the forests make the genus *Macacus*, or macaques, which during the Pliocene spread pretty well all over Europe, even up into England, and also into western Asia where they still live. In the Pleistocene some representatives of the genus went with the great wave of migration from southern Asia into Africa, but they have become extinct in that continent except for one species, the Barbary ape. Those members of the group which left the woods and took to the more open country developed great strength and powerful jaws and are the baboons (*Cynocephalus*). These too originated in southern Europe and migrated during the Pliocene eastward into Asia, and during the Pleistocene down into Africa, to which continent they are now confined.

The second subfamily of the Cercopithecidae are the langurs (*Semnopithecus*, *Nasalis* and *Colobus*) which, while remaining largely arboreal, have specialized as her-



→ Cercopithecus line — Cynocephalus Macacus line Semnopithecus line

FIG. 3. Diagram of the radiation of the quadrupedal old world monkeys.

bivors among the primates, feeding exclusively on leaves; in response to which they have developed a stomach of several pouches comparable to that of a sheep or cow. Like the preceding subfamily, they originated in southern Europe and during the Pliocene moved over into southern Asia, where the langurs and the nasal monkeys live to-day. Some members of the group, however, moved during the Pleistocene in Africa where the thumb was much reduced, which feature distinguishes the genus *Colobus*.

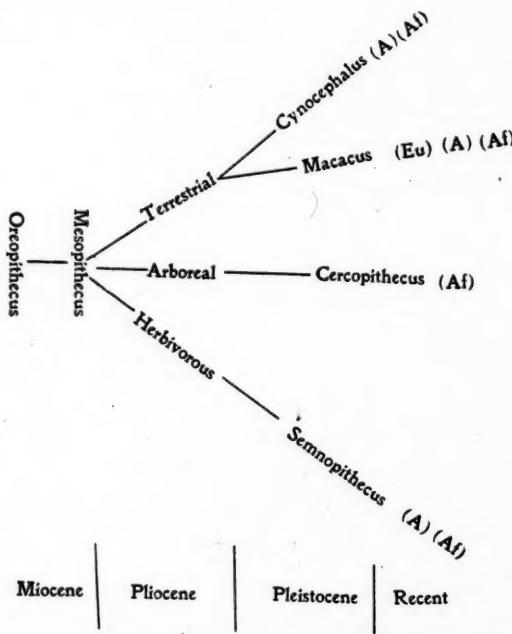


FIG. 4. Genealogical relationships of the quadrupedal old world monkeys.

Lastly the unspecialized subdivision of the family, the *Cercopithecus* genus, followed the same lines of migration and reached Africa where they now live, having spread over the major part of the continent.

Turning back to the early Miocene, we find that there

was another group of primates which tended to come to the ground, and these tended to assume a more or less upright position, with a bipedal gait. The hands thus free to take hold of objects, were free to develop a deftness and adaptability, which seems to be the key to the progressive development of the apes. It seems however that this handling of objects (food, sticks, stones) began before they left the trees and was really the cause of taking the bipedal gait. The climbing offered an ever changing grasp and carrying food to the mouth was a natural starting point; so that, with the front paws used as hands, there is a good reason for exempting them from the heavy work of locomotion. Contributory to this idea is the eolith development. These crude flaked flints⁸ begin back in the Miocene at least, and as Penck⁹ suggests the only known primate which might be suggested as an eolith-maker is *Dryopithecus*. It seems highly probable then that the hands had begun to be used as such before the first apes came to the ground and that this specialization of the hand was the cause of the upright position and bipedal gait. Of course the varied experience resulting from taking up all sorts of objects and using them for different purposes tended to develop the intelligence, and that furthered handling, the two acting and reacting on each other.

In the early Pliocene of southern Europe three divisions of the simian group have already arisen,¹⁰ one group remaining arboreal, or more probably reverting again to the trees, a second group developing great muscular and skeletal strength, the third group developing especially the brain and central nervous system.

The first of these groups, *i. e.*, the retrogressive or aboreal group, is represented in the Upper Miocene of southern Europe by *Pliopithecus*, a form ancestral to the modern gibbons, and one which during the upper Miocene

⁸ See MacCurdy, *Amer. Anthropol.*, Vol. 7, n. s., pp. 425-479, 1905.

⁹ *Science*, Vol. 29, n. s., p. 359, 1909.

¹⁰ See Schlosser, *Zoologischen Anzeiger*, Vol. 22, p. 289, 1900.

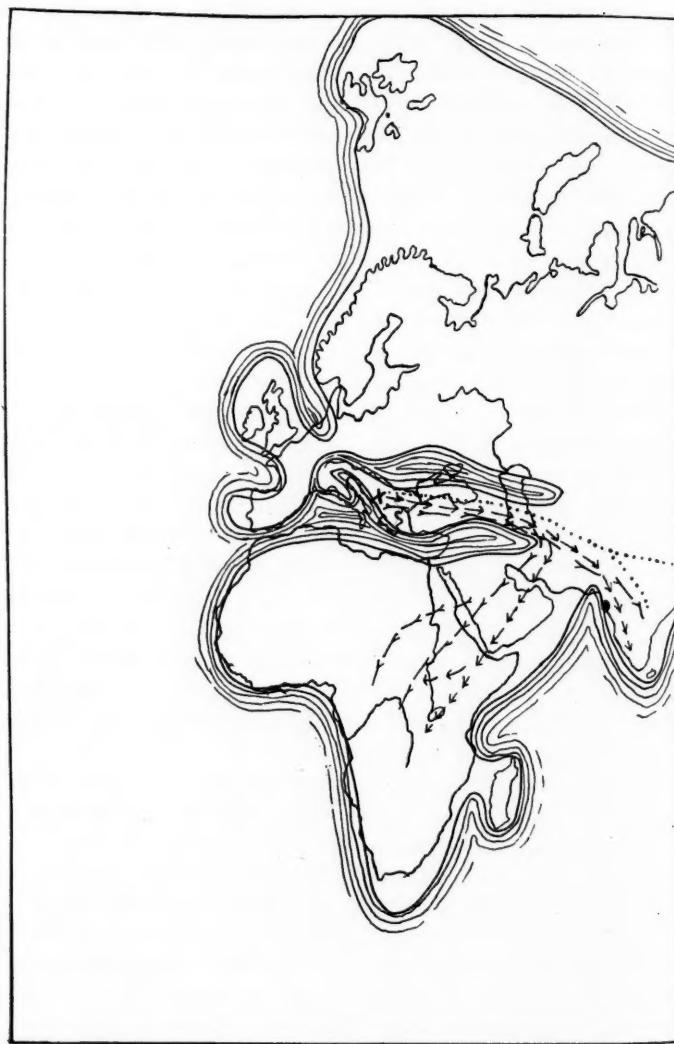


FIG. 5. Diagram of the radiations of the bipedal primates or apes.

and lower Pliocene spread over a large part of Europe (France, Germany, Switzerland). However when the colder climate of the Pliocene developed, the European contingent was exterminated, and only those in Asia have survived as the gibbons of to-day.

The second group which developed especially strength is rare both in prehistoric and recent times: but it seems

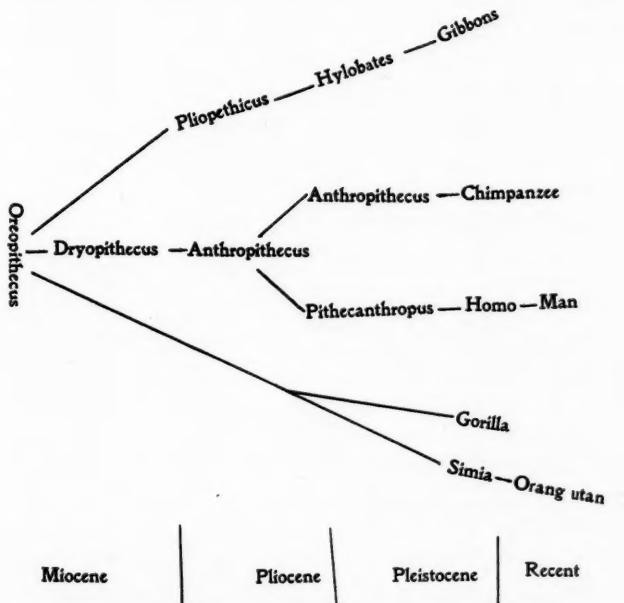


FIG. 6. Genealogical relationships of the anthropoid apes.

to have originated first in southern Europe though no representatives have yet been found. In the Pliocene however the genus *Simia* has been found in southern Asia where its representatives still remain as the orang utan. During the Pleistocene representatives of the group seem to have reached Africa where they have persisted as the gorilla.

The third group has at its base *Dryopithecus*, the middle and upper Miocene ape which ranged over a con-

siderable part of Europe: but at the end of the Miocene this form became extinct and with it the last of the chimpanzee line in Europe, the next representative being *Anthropithecus*, the true chimpanzees, found in the Pliocene of India. This first chimpanzee makes a slightly closer approximation to man than the living species. During the Pleistocene the wave of immigration into Africa included *Anthropithecus*, which has survived only on that continent. In 1896 Dubois found in Java in beds now generally called early Pleistocene, the top of a skull, a femur, and a few fragments of a transitional form which is in many ways like the apes and in others like man. This he called *Pithecanthropus erectus*, and it stands as either a very high grade ape or as a low grade man, the latter being the usual designation. If not the actual ancestor of man, it is at least a typical stage in his development.

From the distribution of *Anthropithecus* and *Pithecanthropus* it seems certain that man originated in southern Asia, at least by the beginning of the Pleistocene: and that he radiated from there westward across Europe where such remains as the Heidelberg jaw, and those of the Neanderthal type have been found so widely. He probably also migrated easterly into North America, and thence south with the Pleistocene fauna into South America where very primitive remains have recently been described by Ameghino as *Diprothomo platensis*, and *Homo pampensis*. Remains have been strangely scarce in North America, though the fauna, with which early man usually associated is present in various parts of the continent.

JEAN MARCHANT; AN EIGHTEENTH CENTURY MUTATIONIST¹

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JEAN MARCHANT was the son of Nicholas Marchant (died, Paris, 1678), director of the Jardin du Roi, the principal author of the famous "Mémoires pour servir à l'histoire des Plantes," published in 1676 under the auspices of l'Académie royale des Sciences and edited by Dodart.²

The name Marchant is perhaps most familiar in connection with the genus *Marchantia*, which, though not unfrequently attributed to Linnæus, was named by Jean Marchant in honor of his father,³ when, because of his discovery of the "flower" and "seeds," he removed it from the genus *Lichen*, under which it formerly had figured as *Lichen petræus stellatus*.⁴

¹ Contribution from the Botanical Laboratory of the University of Michigan, No. 121.

² Clos, D., "Les trois premiers botanistes de l'Académie royale des Sciences, Dodart et les deux Marchant," *Bull. Soc. bot. France*, 35: 285, 1888. In this paper Clos goes to great length in a successful attempt to show that Nicholas Marchant was the chief contributor to the work just referred to, though a simple reference to a statement on the part of the academy and which curiously enough seems to have escaped him, would at once have settled the matter beyond question. In "Table Alphabétique des matières contenues dans l'Histoire et les Mémoires de l'Académie Royale des Sciences," publiée par son ordre, 1: 200, 1666-1698, 1778, one reads: "Marchant [M. Nicholas] a fourni tout le Botanique des Mémoires pour servir à l'Histoire des Plantes. T. 4, p. 122."

³ Marchant, J., "Nouvelle découverte des fleurs et des graines d'une plante rangée par les botanistes sous le genre du Lichen," *Mém. de l'Acad. roy. d. Sc.*, 1713, pp. 229-234. "Nous établirons pour cette plante un nouveau *genre* que nous appellerons *Marchantia* du nom de feu M. Marchant, mon père, qui le premier eut l'honneur d'occuper une place de botaniste dans cette Académie, lorsque le Roy en 1666 crée cette Compagnie."

⁴ Caspar Bauhin, Pin. 362.

It was indeed a discovery far beyond mediocrity and indicative of excellent powers of observation. For since the days of Cæsalpino but little advance seemed to have been made as to the organs of reproduction. Even one of Linnæus's first papers, "Præludia Sponsaliorum arborum," deals with the sexes of plants, and, attracting the attention of Olaf Rudbeck (1729), secured for the "father of botany" *in spe*, the position of assistant.⁵

Darwin, in the introduction to his "Origin of Species," points to Buffon as the first transmutationist, though, as he says, it was the views of Lamarck which first attracted general attention. But long before their time, when Buffon was but a boy of twelve, Jean Marchant had made some very pertinent observations on the sudden origin of species. Believing genera to have been created as such, a view expressed in his early days by Linnæus himself in his "Systema Naturæ,"⁶ he was able to see new "species" originate suddenly. He had at his disposal a garden, probably already used by his father,⁷ who was an ardent collector and introduced the seed of many foreign plants, growing and describing them.⁸ His observations were made upon *Mercurialis annua*, the dog's mercury,⁹ a plant long known as possessing certain reputed virtues.¹⁰

⁵ Wittrock, Veit B., "Nagra ord om Linné och hans betydelse för den botaniska Vetenskapen," *Acta Horti Bergiani*, 4: No. 1, 1907.

⁶ *Genus omne est naturale, in primordio tale creatum.*"

⁷ Presumably the "Jardin du Roi." "Il [Nicholas Marchant] faisait cultiver au Jardin Royal celles qui ne se trouvoient que difficilement à la campagne," *Hist. de l'Acad. roy. d. Sc.*, 1666-1686, 1: 200, Paris, 1733.

⁸ "Hist. de l'Acad. roy. d. Sc.," 1680, p. 307.

⁹ This term is also used for *M. perennis*, in which case the name French mercury is given to *M. annua*.

¹⁰ It was supposed that the juice of species of *Mercurialis*, especially of the Mediterranean *M. tomentosa*, had the power to determine the sex of children, according to whether the mother drank the juice of the male or of the female plant. Unfortunately the true sex of the plant was not known, as also is apparent from Marchant's paper. Thus boy's mercury was the name applied to the female plant of *M. annua*, girl's mercury the name given to the plant with staminate flowers. Also, staminate flowers repeatedly have been observed on the pistillate plant (*f. ambigua*, Duby, "Bot. Gall.," 1: 417).

In 1715 Marchant noted in his garden¹¹ the appearance of a laciniate form of *Mercurialis annua*, which he designated *Mercurialis foliis capillaceis*. The next year, in the same part of the garden, this plant reappeared, being represented by four individuals. There appeared further two plants, the foliage of which, though also of a laciniate character, was sufficiently different to permit of their being readily distinguished. To these plants he applied the name *Mercurialis foliis in varias et inæquales laciniias quasi dilaceratis*. The description of the leaves at once leads us to recognize this plant as a typical laciniate form, especially his reference to "a large number of leaves, which, because of their irregular outline, resemble mere remnants of leaves torn or gnawed by caterpillars," curiously enough, the same expression which I used to describe the appearance of the leaves in the flowering shoot of *Arctium minus laciniatum*.

In an attempt to explain the successive appearance of these two new forms of *Mercurialis* we could assume that both forms had been created in 1714, but that the seed of one had germinated in 1715, while that of the other remained dormant for a year. It is, however, far more probable that but one plant of the first laciniate variety had been allowed to grow up, and that in the next year special orders were given to the gardener who had the care of this particular portion of the garden, to allow to grow all seedlings which in the least resembled those of *Mercurialis*. Still another possibility is that the first form, created in 1714, gave rise, in 1715, through a second mutation, to another, less laciniate form, which appeared in 1716.

After reporting these plants in 1719, Marchant makes no further mention of them, but de Candolle¹² refers to them, under *Mercurialis annua*, in the following terms:

¹¹ Presumably the Jardin Royal. "Jean Marchant avait, ainsi que son père, le titre de directeur de la culture des plantes du Jardin du roi." Michaud, "Biog. Univ.," 26: 486, 2d ed.

¹² "Prod.," 15: 797.

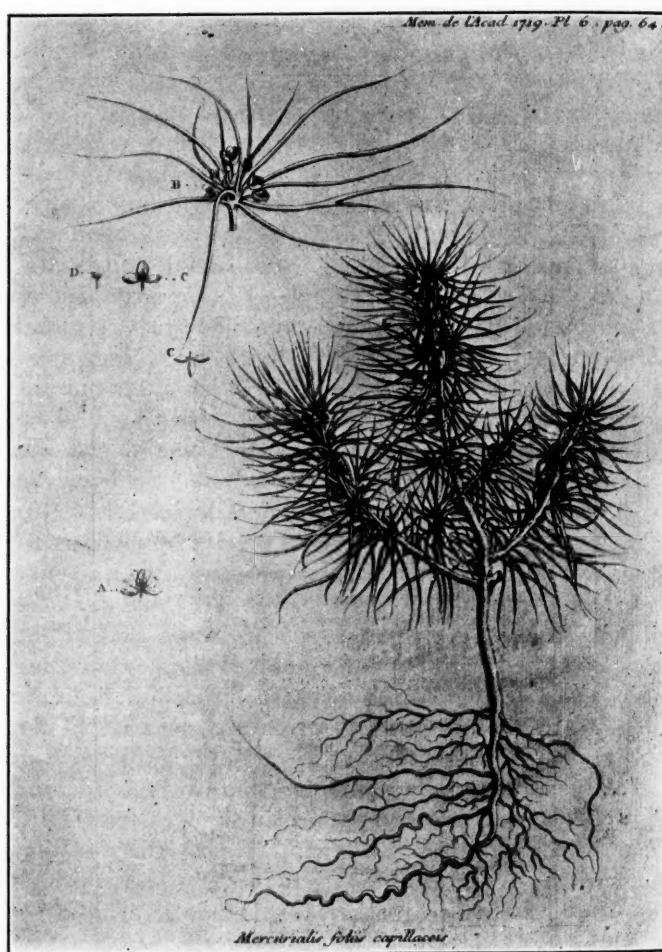


FIG. 1.

Monstroso occurrit: 1° *laciniata*, foliis laciniato-dissectis. ⊖ In Gallia (Marchant). — March. in *Act. Acad. Paris*, 1719, p. 59. t. 6. — 2° *capillacea* (Guep. "Flore Maine-et-Loire," ed. 3, p. 401), foliis ad laciniias auguste lineari-lanceolatas, lineares v. capillaceas integras reductis. ⊖ In hortis Andegaviæ et ad Issy-l'Evêque (Guepin, *l. c.*, Gren. et Godron,



FIG. 2.

"Flore de France," Vol. 3, p. 99). — Marchant in *Mém. de l'Acad.* Paris, 1719, p. 64, t. 6.

This discovery on the part of Marchant is particularly interesting for various reasons. In the first place be-

cause it is the second historical case of mutation on record. Further, because of a certain analogy with Sprenger's discovery of *Chelidonium majus laciniatum*,¹³ Sprenger, an apothecary of Heidelberg, cultivated a large number of plants in his garden as was the custom in those days, and, about 1590, observed there a type of *Chelidonium majus* formerly unknown to him and to which he gave the name *Chelidonia major foliis et floribus incisis*. Tournefort, in his "Schola botanica," mentions three forms, *Chelidonium majus vulgare* C. B., *Ch. majus foliis querinis* C. B. (*Ch. folio laciniato* J. B.) and *Ch. majus foliis et flores minutissime laciniatis* Hort. reg. par. The second of these was identical with the variety discovered by Sprenger, the third a form which had originated in the Paris Botanic Garden and was distinguished from the other by the greater reduction of the leaf blade. Of the laciniate varieties of both *Chelidonium majus* and *Mercurialis annua* there exist therefore two forms. While those of *Chelidonium majus*, and especially the first, are fairly well known and may be encountered in almost any botanic garden, it is not so in the case of either of the laciniate varieties of *Mercurialis annua*. At least I do not remember seeing them, nor do I recollect the occurrence of their names in the seed-exchange lists annually published.

In at least one instance the claim has been made that one of the laciniate varieties of *Chelidonium majus* originated *de novo*¹⁴ a claim which to Korschinsky¹⁵ appears to lack foundation. No such claim has been made for one of the laciniate varieties of *Mercurialis annua*. However, as seen from de Candolle's account, given above, at least one of the laciniate forms was reported as occurring both in the botanic garden at Angers and also at Issy-l'Évêque. These two towns, the first in Maine-et-Loire,

¹³ Roze, E., "Le *Chelidonium laciniatum* Miller," *Journ. de Bot.*, 9: 296, 1895.

¹⁴ Clos, D., "Réapparition de la Chéridoine à feuille de Fumeterre," *Compt. rend.*, 115: 381, Paris, 1892.

¹⁵ Korschinsky, S., "Heterogenesis und Evolution," *Flora*, 89: 240, 1901.

the other in Saône-et-Loire, lie more than 200 miles apart. It is possible, perhaps, that the seed was brought from Paris to Issy-l'Évêque and from there transported accidentally to Angers. Or is it more probable that the variety originated a second time? It is a question which forever must remain unanswered, though it is not improbable that a French student, who most readily commands the means of research in this direction, could, by delving into historical records, perhaps throw some light upon the subject.

The work of Jean Marchant gives evidence, not only of great exactitude, but also of excellent powers of observation. Thus, a few years after his discovery of the laciniate forms of *Mercurialis annua*, he was able to give an account, and, as I believe, the first, of a myxomycete, of the "flowers of tan."¹⁶

As is evident from the description and drawings, he was dealing with a *Fuligo*. He recognized the vegetable character of the organism, but unfortunately places it with the sponges, giving it the name *Spongia fugax, mollis, flora et amoena, in pulvri coriari nasceus*. In this case, as in the other, Marchant's ambition did not lead him beyond a mere, apparently most accurate, description, something decidedly pleasing when we remember that in the same year Jean Marchant was elected to membership of the Académie (1678), there appeared Father Kircher's *Mundus subterraneus*,¹⁷ and that one of his contemporaries was de Maillet (1656-1738), who "derived birds from flying fishes, lions from sea-lions, and man from *l'homme marin*, the husband of the mermaid!"

Believing genera to have been created as such, Marchant did not go beyond this point, but realized that species were derived from preexisting ones. His own

¹⁶ Marchant, J., "Observation touchant une végétation particulière qui naît sur l'écorce du chêne battue, et mise en poudre, vulgairement appelée du Tan," *Mém. de Math. et de Phys. de l'Acad. roy. d. Sc.*, 1727: 335, Paris, 1729.

¹⁷ Osborn, H. F., "From the Greeks to Darwin," 109, 1908. "The worthy priest describes orchids giving birth to birds, etc."

words should be quoted here. As far as I am aware, there exists but one account of any length of Marchant's discovery, that of Korschinsky.¹⁸ Godron refers to him¹⁹ as does de Vries.²⁰

Since the original papers are rather inaccessible to the majority, it was deemed of interest to give here a translation of Marchant's two articles dealing with his discovery. The translation of course has been made as literal as possible. The first paper is merely a résumé of an address made before the academy by Marchant. The second gives a detailed account as published in the *Mémoirs*.

ON THE PRODUCTION OF NEW SPECIES OF PLANTS²¹

In the month of July, 1715, Mons. Marchant noted in his garden a plant which he did not know, and which attained a height of from five to six inches.²² It persisted until the end of December, when it dried up and died. He believed to be able to class it only with the genus to which the mercury belonged; and since it was entirely new and thus far had not been described by authors, he called it *Mercurialis foliis capillaceis*.

The following year in the month of April, and in the same place where this plant had been, he saw appear six others, of which four were quite similar to the former, and two others sufficiently different to make another species of mercury, which he named *Mercurialis foliis in varias & inaequales lacinias quasi dilaceratis*. It persisted until the end of December, in which respect these two species are different from the common mercury, which, though annual like these, does not last as long.

¹⁸ *Loc. cit.*

¹⁹ Godron, "De l'espèce," 1: 160 (not seen).

²⁰ De Vries, H., "Die Mutationstheorie," 1: 136, 1901. "*Mercurialis annua lacinata* ist 1719 von Marchant als neue Form entdeckt worden; sie ist seitdem samenbeständig geblieben."

²¹ "Hist. de l'Acad. roy. d. Sc.," 1719, p. 57, Paris, 1721.

²² The French "pied" is equal to 1 foot 1 $\frac{1}{2}$ inch of our measure and is divided into 12 "pouces," each "pouce" being divided into 12 "lignes." Pouce and ligne are translated inch and line, respectively.

These two new plants since have multiplied in a space of seven or eight feet, and, which is astonishing, never has Mons. Marchant been able to discover any signs of seed upon them. At the same time the slight extent of the plot upon which they reappear every year sufficiently shows that they must have been derived from seeds which probably fell upon it from preceding plants. Since some time ago were discovered the secret means which several plants make use of to hide their seed, it is all the more marvelous that there still are some which can succeed in hiding them.

But the principal reflexion of Mons. Marchant upon these two plants is that it would not be impossible for new species to be formed; for these have all the appearance of being such; how else could they have escaped all botanists? Art, culture and, still more, chance, that is to say, certain unknown circumstances, every day bring about novelties in interesting flowers such as the anemones and buttercups, and these novelties are treated by botanists as varieties only, which do not deserve to change the species; but why should nature be incapable of novelties which went thus far? It seems she is less constant and more diverse in plants than in animals, and who knows the limits of this diversity?

At this rate the old-time botanists would not have been wrong in describing so few species in a single genus; they were not acquainted with more, and it is time which has brought new ones. For the same reason the future botanists would be overwhelmed, and finally obliged to abandon the species to limit themselves to the genera. But ere forecasting that which will be, one must assure oneself of that which is.

OBSERVATIONS ON THE NATURE OF PLANTS

BY MONS. MARCHANT²³

The researches which are made in Natural Science frequently demand long series of observations to arrive at a perfect knowledge of the sub-

²³ "Mém. d. Math. et de Phys. tir. d. Rég. de l'Acad. roy. d. Sc. de l'Année, MDCCXIX," p. 59, Fy. 1, 1719, Paris, 1721.

jects which one undertakes to deal with; and it is for this reason that one must not be astonished that the botanists have not yet discovered the seeds of a number of plants, though they knew these plants long since. One must be the less surprised about this since among these plants, there are several which can not be cultivated, and with which one meets accidentally only, and others which, because of their smallness, also bear only exceedingly small seeds, which frequently escape even the most observant. The observation of which we are about to speak is related to these kinds of investigations. It is in fact a matter of two plants which we deem to be of the same genus, but of different species, of which during the four years which we have known them, which we have studied them, it has been impossible for us to observe the seeds. These plants showed themselves to us for the first time in our garden; I had never seen them before, neither there nor in other places, and I do not know that botanists have made mention of them.

Yet they are sufficiently tall to be observed by those who apply themselves to a knowledge of the Simples, if they grew commonly in our gardens, just as are a number of other common plants; these new plants finally have reproduced themselves in our garden, from the time above mentioned, without one having been able to find seed upon them.

According to the smell of these plants and the structure of their flowers, I am satisfied they belong to the genus of the Mercury of which we here represent the flower (Fig. A, Pl. 1). To make known the nature of these herbs, we will begin by describing the first species which we observed in the month of July of the year 1715, and we shall name it *Mercurialis foliis capillaceis*.

The plant was five to six inches high, its stem was about two lines in thickness, bare at the base, round, of a pale green color, smooth, shiny and nearly transparent, provided with five branches, two of which, parallel and placed towards the base of the stem, were longer than two inches. The others were unequally shorter. The stem and the branches were rather irregularly beset with leaves without petioles, some alone and bare (*les unes seules et nues*), the others attached in bunches and intermixed, accompanied at their point of origin by several flower buds, which together, or leaving some spaces between them, surrounded the stem and the branches of this plant. The longest leaves ending in a sharp point measured about one inch, their width was half a line in the middle, some were wider at their base; some others were dissected towards the point in two very narrow strips of different lengths, and all of them were of a green-brown color, smooth, shiny and slightly indented along their length. These leaves pointed in this or that direction without order, some with the tip turned upward, the others downward, and others bent in sickle-like form. The smallest were placed horizontally. All were rather stiff, notwithstanding their delicate texture, and they appeared more or less like the principal veins of leaves

which had been stripped of their lateral veins and of their parenchyma. The flowers which were collected in small clusters (*B. nat. size*) opened successively only. Their color approached a greenish yellow. They consisted of a calyx of three leaves *C* of an oval shape, shell-like, separated from each other at their point of origin by a cluster of ten or twelve very fine and very short filaments *D* which had no tips.²⁴ The flower was but a line in diameter and its pedicel was so short that it was hardly visible.

The root of this plant was a little less thick than its stem. It was from four to five inches long, twisted, forming slight undulations throughout its whole length, accompanied by several fibers similarly undulating and hairy, which branched off in all directions. Its internal substance was very white, hard and covered with a more or less fleshy bark of a white-yellow color.

The entire plant on being rubbed had a rank unpleasant odor and a nauseous flavor having something nitrous about it, closely approaching the taste of the common Mercury.

This plant persisted until the end of the month of December, after which it dried up and perished entirely.

This observation put me on the alert to discover this plant the following year. Until the end of the month of March I noticed nothing of that for which I was looking; but in April I saw appear six plants, of which four seemed to be the plant which just now has been described. The two others were slightly different from the preceding, in that they had larger leaves. These and the others increased in size, and I had the pleasure to observe among these six plants a second species which as yet was unknown to us, as will be seen from the following description; and since this time these two species of plants reappear every year, without cultivation in the same portion of the garden. We shall name this second species, *Mercurialis altera foliis in varias & inæquales lacinias quasi dilaceratis*.

It produces a root three or four lines in diameter and six inches long, knotty, provided from its upper portion with several fibrous roots of the same length, much contorted in small equal undulations, which twist towards the bottom of the soil, covered by a quantity of hairy fibers, which extend laterally around the root. Their surface is composed of a chapped, fleshy pellicle of a dirty-white color, which covers a coriaceous and very white woody body, without the slightest indication of pith. From this root there arises immediately a stem about a foot high, a little less thick than the root, hard and woody, covered by a fleshy bark, shiny and smooth, of a pale green color, containing a greenish pith. This shoot from its point of origin to its extremity is furnished

²⁴ " . . . qui n'avoient point de sommets"; sommets, transl. tips, anthers, apices of *Dodoneæus*.

with a number of branches, which together form a sort of very small tufted bush, about eight to ten inches in diameter. These branches are arranged upon the stem without regular order, just as the other little branches which they bear, which frequently are subdivided; and nearly all these branches are slightly thickened at their point of origin. An infinite number of clusters of leaves, arranged along the branches and their divisions and subdivisions, covers this little bush. The leaves resemble each other but little. The larger ones, placed near the base of the branches are an inch long and more, measuring one, two or three lines at their greatest width, and in the whole lot there are some which do not measure a third of the width of the preceding. They are all without petioles, and terminate in a very sharp point, having but very slight traces of veins in the middle, which nevertheless are firm and hard, in comparison with their thickness.

The larger of these leaves have as many as four or five incisions. Some are very deep, forming sharp reentering angles and rounded lobes (*formant des angles, rentrants aigus, & des saillants arrondis*). The other incisions, on the other hand, have obtuse reentering angles and very sharp lobes (*les angles rentrants obtus, & les dentelures saillantes fort aiguës*). Several others of these leaves have but one or two incisions, or even but slight crenations not very deep or undulating; and finally a large number of leaves because of their irregular outline, resemble remnants of leaves torn or gnawed by caterpillars.

From the center of each cluster of leaves appears a mass of from twelve to fifteen or twenty flowers collected together, from between which there arise some small, simple and very narrow leaves, in the manner of small strips terminating in a sharp point.

The flower *A* (natural size, Pl. 2), of which the pedicel is very short, consists of a calyx composed of three small leaves (Fig. *C*) of an oval shape, shell-like, of a green-yellow color. In their axils occur from twelve to fifteen filaments *B* of the same color, without tips and two of these filaments always diametrically opposite, being twice the length of the others. The full-blown flower is about one line in diameter. The entire plant is of a brown-green color, mixed with a yellow tinge. Its taste is insipid with a slight nitrous flavor.

These two plants are annuals; at the same time they last much longer than the common Mercury, since they have been observed to germinate in April, and remain green until the end of the month of December.

They resemble each other greatly because of their flowers, their smell and the consistency of their leaves; but they differ exceedingly by their aspect, and, as has been said, in regard to the outline of their leaves.

The opinion commonly held by botanists is that the male Mercury bears seeds and produces no flowers, and that it produces equally the male and the female Mercury, instead of which the female Mercury

bears sterile flowers only, that is to say, that it does not produce seeds.

On this principle, our two new plants on which we have seen flowers only, would be female Mercuries, resulting from seeds of the male Mercury; but under these circumstances it would be rather difficult to understand why these two plants reappear in this garden only in the same space of seven or eight feet of ground, where they were discovered the first time, since the common male and female Mercuries germinate and grow in great abundance in all the gardens; which would lead one to believe, contrary to the opinions of modern botanists, that the two plants of which we here give descriptions, bear seeds, because they reproduce themselves without aid in this spot, for certainly they are not found anywhere else.

We shall continue our observations on this phenomenon; and in the meantime we shall propose a few conjectures on the large number of species we believe plants may give rise to.

The men of science who are interested in plant culture, and especially those who are fond of plants which bear beautiful flowers, such as the anemones, tulips, carnations and other flowers, well know that the seeds of these plants, being sown, frequently produce pleasing and interesting diversities. Nature, without regard to the beauty of the flowers, behaves in the same manner in the diversity of the species of which it increases the number in the Herbs or Simples.

The instance of our two plants indicates this sufficiently, since in four years we have witnessed the birth of two constant species which were unknown to us.

Because of this observation there would be reason to believe that the Almighty once having created individuals of plants as a model for each genus, made up of all imaginable structures and characters, capable of reproducing their like, that these models, I say, or types of each genus, in perpetuating themselves, finally had produced varieties, among which those which have remained constant and permanent, have constituted the species, which as time went on and in the same manner, have made other different productions which have so increased Botany in certain genera, that it is a fact that to-day there are recognized in some genera of plants as many as one hundred, one hundred and fifty, and even as many as more than two hundred distinct and constant species belonging to a single genus of plants.

The proof of that which is suggested on the subject of the production of species seems all the more well founded, when one remembers that the most ancient botanists have mentioned but about four hundred types of genera of plants, to which they add few species, which leads one to believe that at that time the species were as yet not very numerous; since at present we know more than eight hundred types of genera, to which must be added thirteen or fourteen thousand species or more,

among which, it is true, several are mere repetitions, and others are but simple varieties.

One may be sure that botanists, in going through countries which formerly were unknown, will discover new types of genera of plants from the point of view of the physicians, though created many centuries ago, as well as an immense number of species which will have arisen from them. Which, in time to come, and according to the conjectures made above, would tend to reduce Botany to a knowledge of the types of the genera only, giving up the species, to evade the confusion to which they might give rise in this science.

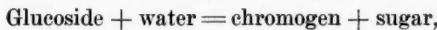
Yet whatever might result from this, we must profit by the new discoveries, to illustrate the history (evolution) of plants.

The species may have still other uses, though it is true that the majority of plants, of which use is made in medicine, are ordinarily those which are called type of the genus, because they are the first in which certain virtues and distinctive characteristics have been recognized, which constitute each genus of plants; but since because of lack of genera, species frequently are substituted for them in Pharmacy, one must not neglect a knowledge of the species, even though there are no living organisms in Nature which increase more abundantly and who make more diversities than do plants, as has been said, always if one excepts Insects, which apparently reach a greater number, but which are not so well known to us as are plants.

NOTES AND LITERATURE

NOTES ON HEREDITY

MISS WHELDALE presents an interesting theory concerning the formation of anthocyanin.¹ The theory for which a good deal of evidence is presented is that of the reversible reaction



which is controlled by one or more enzymes.

Whether this reaction shall go forward or backward depends upon the relative amount present of the various substances entering it and upon the nature and quantity of the enzymes which bring about the reaction. She points out that certain species having flowers, leaves, etc., slightly or not at all colored give rise to colored varieties which are recessive to the type. These facts are explained if we assume that the colored forms arise because of the absence in the color tissue of the enzyme which causes the synthesis of the glucoside. Under these conditions, chromogen which is formed would be free and available for oxidation into pigment. In the type the enzyme is supposed to be present, and the condition of equilibrium is such that all, or nearly all, of the chromogen unites with sugar and hence can not be oxidized.

In the *Journal of Morphology* for March, 1911, Wilson gives a very interesting discussion of the chromosome behavior in *Nezara*, following which he gives a discussion of the possible ways in which the idiochromosomes may be changed in number.

One of the most interesting things in this paper to the student of genetics is the high degree of probability which Wilson shows to exist that the behavior of the idiochromosomes is not only accountable for sex inheritance, but for the inheritance of certain sex-limited characters. He points out that cytological considerations render it probable that the morphological dimorphism of the spermatozoa in certain species has arisen by the transformation of a single pair of chromosomes, producing an XY pair. He also shows that in certain species the XY pair of chromosomes

¹M. Wheldale on "The Formation of Anthocyanin," *Journal of Genetics*, I, No. 2, March, 1911.

may be interpreted as an X chromosome attached to one member of a pair of Y chromosomes, thus one of the members being an XY combination, the other Y.

He then calls attention to the very interesting case of the inheritance of white eye found by Morgan in one of his *Drosophila* mutants. This mutation appeared first in the male. When this male was crossed with the normal red-eyed female the progeny all had red eyes; but in the next generation some of the males showed white eye again. Morgan has shown that in a cross of this kind it is only the female progeny which transmits the abnormality, and although she transmits it to half of both sexes of her offspring it does not appear in the female. It is possible, however, to secure females with white eyes.

These facts are all explainable on the following basis. Let WF represent the X element in which white eye is associated with the female determiner. RF would then represent the X element in red-eyed individuals. The formula for the white-eyed male would be WF.Y. The formula for the normal female is RF.RF. These mated give WF.RF females and RF.Y males; but since R is dominant to W these females have red eyes.

If now these heterozygous females be mated with ordinary males, the following four types of progeny are produced: WF.RF females, RF.RF females, WF.Y males, RF.Y males. Thus half of the males in this generation will have white eyes. But if a heterozygous female be mated with a white-eyed male, half of the female progeny and half of the male progeny will have white eyes.

Wilson points out that these phenomena exactly parallel behavior which he has found in the X and Y chromosome elements of certain species, where the X element plainly consists of an X chromosome united to a Y chromosome, while the synaptic mate is a Y chromosome. If in such a species a mutation like that of the white eye of *Drosophila* occur as a result of the loss of some characteristic on the part of the Y chromosome which constitutes a part of the X element, then such mutation would behave exactly as Morgan found this white-eye mutation to behave in *Drosophila*.

In referring to this parallel in the behavior of the chromosomes and of this mutation Wilson remarks: "So precise is the correspondence of all this with the course of sex-limited heredity of this type that it is difficult to resist the conclusion that we have before us the actual mechanism of such heredity—in other words.

that some factor essential for sex is associated in the X element with one that is responsible for the sex-limited character."

The author points out other important parallels of a similar nature. To the cytologist probably the most interesting part of Wilson's paper is the suggestion he makes regarding the possible origin of the X and Y elements. On the whole, this paper greatly strengthens the hypothesis that there is some kind of definite relation between the chromosomes and Mendelian factors in heredity.

Doneaster, in the *Proceedings* of the Cambridge Philosophical Society² gives a short note on the spermatogenesis of *Abraaxis grossulariata* (currant moth), in which he had previously shown that the *grossulariata* character is a sex-limited, dominant Mendelian factor, not borne by eggs bearing the female determiner. He finds the mitoses very small, and was unable to count the chromosomes satisfactorily, but says there are between fifty and sixty. Two kinds of primary spermatocytes are found, in different follicles. One of these appears to be abnormal and is thought not to be functional in fertilization. There is no evidence that the male produces two kinds (male and female determining) of spermatocytes; such as are found in many species. The number of chromosomes visible in the equatorial plate of the first spermatocyte division, as well as in the second, is twenty-eight. The oogonial mitoses do not differ recognizably from the spermatogonial.

In this moth the female produces two kinds of eggs, one female producing and wanting in the *grossulariata* character, the other male-producing and with or without this character according as the female is of the *grossulariata* or the *lacticolor* type. It is to be expected that the chromosomal provision of the two kinds of eggs would be different; yet, as Wilson and others have shown in some species for males, the differential chromosomes may not be distinguishable. Doneaster's results are consistent with the latter suggestion.

Doneaster also gives some interesting results of a test of a theory of sex inheritance which has gained considerable popular credence.³ The theory is that one ovary produces female-producing eggs, the other male-producing eggs. The test was made on female albino rats. In one individual the right ovary was removed, in the other the left. Both females subsequently pro-

² Vol. XVI, Pt. I.

³ *Journal of Genetics*, Vol. I, No. 1, November 18, 1910.

duced offspring of both sexes. Post-mortem dissection of the two females showed that the operation of removing an ovary had been entirely successful, and new ovarian tissue had not been regenerated. These results disprove the theory so far as concerns these animals.

In the March number of the *Zeitschrift für Induktive Abstammungs- und Vererbungslehre* Dr. Gates presents a very interesting paper on the inheritance of pigmentation in *Œnothera*. He shows that some quantitative variations in the pigmentation of the calyx of *O. rubrinervis* are not hereditary, while other quantitative variations are hereditary. One striking case of a mutant which produces an increased amount of anthocyanin was found in his investigations.

To most students of genetics the most interesting part of Dr. Gates's paper will be his discussion on pages 367-8 of the probable nature of the differences between genotypes which furnish us our Mendelian character pairs. The evidence he presents is strongly against the assumption that all of these hereditary differences are due to the loss or origination of definite cell organs which can be looked upon as the physical basis of the separately heritable characters. Rather, "Everything points to its being what it appears to be, namely, a positive variation in some physiological factor concerned in anthocyanin formation. It is not necessary to think of a form as having lost an inhibitor, but rather has there been a quantitative readjustment of the relation between the substances which by their chemical interactions produce anthocyanin, and those which decompose it as soon as formed, or which by their presence divert the metabolic processes and bring about chemical reactions of a different sort."

He states that the appearance and behavior of the mutant above referred to (*O. rubricalyx*) can not be explained on the basis of the presence or absence of any factor or substance in this mutant which is not also present in *O. rubrinervis*. "Obviously the germinal change has been rather in a quantitative readjustment, and it would seem that many, though not necessarily all, the cases of Mendelian color inheritance are explainable on a similar quantitative basis rather than by a presence-absence hypothesis."

He calls attention to the well-known fact that, especially in organic chemistry, many reactions may go in quite different directions according to the relative amounts of the reacting substances present, and suggests that many of the phenomena of

heredity are due to changes in the relative amounts of substances produced by the metabolic activities within the cell.

Even white varieties, which are frequently referred to as having lost the factor for color, may originate merely from a change in the quantity of some substance or substances present. That many white flowers do not owe their lack of pigment to the absence of a pigment-producing factor is shown by the fact that such flowers frequently show pigment in exceptional individuals, and many white flowers develop more or less pigment with age. Presumably, as the flower passes its prime, there may be changes in the rate at which various metabolic processes go forward, and this may induce pigment formation.

In *Science* for May 31, 1911, Professor Morgan gives some interesting data concerning the origin of a number of mutations in *Drosophila*. In a culture treated with radium one fly was produced the marginal vein of whose wings was beaded, and this character appeared in a very small proportion of the next generation. The proportion of beaded wings increased from generation to generation until a stock was produced that gives, in certain cultures, nearly 100 per cent. of the new character.

In the seventh generation of the beaded-wing stock a fly appeared with the end of the wings cut off nearly squarely, and indented at the ends, or somewhat scalloped. This character is confined almost exclusively to the female line. In the next generation twenty-one flies, with truncated wings, appeared along with 230 having normal wings. In the third generation some of the truncated-winged flies produced nearly 50 per cent. of truncated wings.

In the second generation of the beaded-wing flies a male appeared with wings shorter than the abdomen. A similar mutation occurred in a related stock. These abnormal flies bred together have produced 964 normal males and females, six short-winged males and two females.

In the seventh generation of the beaded-wing stock a fly appeared with wings like the normal in form but extending no further than the end of the abdomen. This character proves to be sex limited.

Occasionally flies have appeared, especially in the truncated-wing stock, with each wing swollen up to a balloon or a bladder filled with fluid. Practically a pure stock of this mutation is now on hand.

Several times flies have appeared that failed to develop black pigment in the body. These flies have produced no offspring.

A melanistic mutation has also occurred, and a pure stock of it was easily produced.

A male appeared in the melanistic stock having gold and yellow wings: "In fact, the entire fly is conspicuously yellow. This color proves to be sex-limited. Thus far it has appeared only in the males."

Professor Morgan has undoubtedly uncovered some new and interesting facts, and it is hoped that he will be able to discover something concerning the underlying causes of these mutations.

In *Science*, May 19, 1911, Loeb and Bancroft report experiments on the production of mutants in *Drosophila*. They secured four types of mutation; a dark form, a pink eye, a white eye, and a short-winged form. The dark form and the pink-eyed form occurred also in control cultures, and the authors think it is probable that the white-eyed mutation originated before the treatment. The short-winged mutants have appeared thus far only in cultures treated with radium, but in only two out of several hundred such cultures. The treatments used were high temperatures, exposure to radium, and exposure to Roentgen rays. Where mutations were obtained in the treated cultures they occurred in only a very small proportion of the cultures, and the authors appear to doubt whether or not the treatment was responsible for the mutations.

Professor Cockerell, in the *Annals and Magazine of Natural History*, Series 8, Vol. VII, April, 1911, describes a very interesting case in which apparently a female bee, in a genus (*Megachile*) in which the female appears to be heterozygous for the secondary sexual characters, with the female characters dominant, has lost these secondary sexual characters and assumed those of the male. He proposes a new genus (*Androgynella*) for this marked variation from allied forms. Cytological study of these two related genera might reveal something of marked interest.

I wish to draw special attention to the very able article by Professor Michael F. Guyer which appeared in this journal for May, 1911, under the title "Nucleus and Cytoplasm in Heredity." Professor Guyer in this article gives the best presentation I have seen of the relation of the various parts of the cell to the phenomena of heredity. It is not necessary to review the paper here in full, since it is available to the readers of this journal.

W. J. SPILLMAN.

